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THE

BRAIN AND ITS PHYSIOLOGY;

A

CRITICAL DISQUISITION

ON THE

METHODS OF DETERMINING THE RELATIONS

SUBSISTING BETWEEN THE

STRUCTURE AND FUNCTIONS OF THE ENCEPHALON.

BY

DANIEL NOBLE,

MEMBER OF THE ROYAL COLLEGE OF SURGEONS OF ENGLAND.

Inter has doctrinas de fœdere, sive consensibus animæ et corporis, non alia fuerit magis necessaria, quam illa disquisitio de sedibus propriis et domiciliis, quæ singulæ animæ facultates habent in corpore, ejusque organis.

BACON. *De Augmentis Scientiarum*, Lib. iv., Cap. 1.

LONDON:
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1846.

Noble

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TO

JOHN FORBES, M.D., F.R.S.,

PHYSICIAN IN ORDINARY
TO HER MAJESTY'S HOUSEHOLD, AND PHYSICIAN EXTRAORDINARY
TO H. R. H. PRINCE ALBERT, &C. &C.

Dear Sir,—Having, during an acquaintance of some years, had frequent occasion to remark the great interest you take in all subjects connected with the improvement of our methods of research, and the more exact appreciation of scientific evidence, especially as affecting medicine and the collateral branches of knowledge, the idea at once occurred to me, when I undertook the present work, of dedicating it to you; and its chief object being the critical examination of the various methods of inquiry hitherto pursued in elucidating perhaps the most important department in physiology, the functions of the Brain, subsequent reflection has served only to strengthen my conviction that there is no one to whom I could make the dedication with so much propriety as to yourself.

If any other reason had been required to influence my decision, I might have found one of no small weight in the fact, that an article which I wrote at your request, about four years ago, for the British and Foreign Medical Review, touched upon many of the questions more fully discussed in the following pages, and was so favourably regarded by you, that you afterwards reprinted it in the pamphlet form, under the title of *True and False Phrenology*, with a view to

give to it a wider circulation. The same feelings may probably induce you to take a still greater interest in the present treatise.

Perhaps, however, these combined motives might not have sufficed, had there not been superadded a thorough respect for your professional and literary character, and an affectionate regard for your private worth, which all must entertain, who have the privilege of regarding you as a personal friend.

The only reason which, for a time, withheld me from yielding at once to the dictates of my own feelings in this matter, was an apprehension that a dedication to you might be deemed to have originated in a wish to influence, in my own favour, any criticism of the work which might appear in the pages of the Journal over which you so ably preside. But being fully satisfied, on further consideration, that *you*, at least, would acquit me of being actuated by any such motive, the reason in question ceased to have any weight; for even the humblest of your collaborateurs must have had opportunities of perceiving that nothing is to be expected from you, in your editorial capacity, beyond what you conscientiously consider to be strict and impartial justice.

I request you, then, very respectfully, to accept of the present dedication. And that you may long be spared to your profession, and to its literature, to your family, and to your friends, is the sincere wish of, dear Sir, your obliged and faithful servant,

DANIEL NOBLE.

MANCHESTER, *June* 16*th*, 1846.

PREFACE.

The method in which science should be prosecuted, does not form an indifferent circumstance. It is a condition that affects results upon a fixed and determinate principle, and is not an arbitrary matter, or one that can justly be modified according to the caprice or the custom of individuals, or of the age. Method of investigation is to the secrets of nature, what a key constitutes to a treasure that is locked up. If we would not, in the latter case, vainly attempt to gain possession, the *key* must be employed that is fashioned for the intended purpose; and so, in the instance of scientific researches, we are required to follow a method that, in its nature, is calculated to lead to the knowledge which is sought. Nature will but disclose her secrets when rightly invoked.

This truth is now so universally recognised, and in most departments so invariably acted upon, that it may seem superfluous to some persons to reiterate such axioms in reference to scientific inquiry; but, nevertheless, if we refer to a very important branch of physiology, the applicability of this truth is found but too often overlooked. When the discovery of the relations subsisting between the structure and functions of the

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Encephalon is in question, very little attention, in many cases, would appear to be bestowed upon the matter. Each physiologist appeals to his own special pursuit, and very often conceives that by aid of the information which comes from it, he may approximately attain some positive knowledge upon the subject.

"Men are fond of particular sciences and studies, because they have bestowed much pains upon them, and principally applied themselves thereto. And such men as these, if they afterwards take to philosophy and universal contemplations, very often wrest and corrupt them with their former conceits." This aphorism of Lord Bacon accurately expresses a very general state of mind with respect to several branches of physiology. Within the present century, researches into the economy of living beings have been prosecuted with an ardour and an assiduity unequalled in any former age; living animals have been cut and destroyed, diseased structures have been carefully and minutely examined after death, and the brute world has been subjected to a rigid scrutiny; and most valuable facts—much useful information—have been gained in this way. But, accordantly with the long-noted disposition in the human mind when under the influence of the *idola specus*, each labourer has most commonly expected too much from his own vineyard. At all times, indeed, men have referred to their own particular sciences for revelations beyond their capability: mathematicians and mechani-

cal philosophers, in dealing with physiology and with medicine, will render them unduly mathematical and mechanical; chemists, engaging themselves incidentally with these topics, will render them too chemical; and divines, mixing themselves up with natural science, exhibit very often an unreasonable tendency to determine from Scripture what is not always within its purpose.

Each division of natural inquiry is competent to yield its proper results; but when departmental students would deduce from any one branch of knowledge, conclusions that have no direct relation to it, error and confusion will be almost sure to characterise the issue of such attempts. When the vivisector anticipates, from his industry and dexterity, the determination of truths which do not sustain a disclosure by the method employed, he will not fail to be disappointed; and the same may be affirmed of the pathologist and the comparative anatomist. In the many efforts that have been made to shed light upon the functions of particular parts of the Encephalon, the fashion of the age, or the special pursuit, has generally settled the method of investigation; and so, in great measure, the custom continues to the present day. About thirty years ago, mutilations of living animals led to some decisive results in researches into the functions of nerves, and renown became justly attached to the experimenters; hereupon, vivisections for a time came into vogue, and every thing doubtful in physiology was to be tested by the fashionable mode. Later on,

morbid anatomy became the subject of an all-absorbing interest, and thence was inferred an inexhaustible source of widely-extended discovery; how prematurely so, the world is beginning to see. The attention of some of our leading physiologists has of late years been more particularly directed to microscopic examination of the animal tissues, and to comparative anatomy; and these pursuits, as furnishing the means for the solution of all doubtful points in physiology, are, in many quarters, coming into transitory fame. All these sources of knowledge are important; they have each most useful ends or aims, beyond which, however, they cannot legitimately advance. Neither the extent to which they are studied, nor the talent and zeal with which they are investigated, can ever determine their adaptation to the discovery of truths to which they are essentially inadequate. The application of this argument to the particular instance of the Brain and its Physiology, constitutes the subject and purpose of the following pages.

The author thinks it right to state, that almost all the cuts which he has employed are taken from the works of Mr G. Combe; and he has great pleasure in acknowledging the kindness and the promptness with which that gentleman granted his permission to use them.

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THE BRAIN AND ITS PHYSIOLOGY.

CHAPTER I.

INTRODUCTORY VIEW OF THE QUESTION.

The doctrine of the functions of the brain constitutes, most assuredly, a subject of discussion inferior to no other branch of science, either in interest, extent of its objects, or in general importance; and, in physiology, it must, for many reasons, be esteemed the highest department. If anatomical structure and relations furnish any evidence of the consideration to be attached to individual organs of the body, certainly the brain has every title to pre-eminence. Its elaborate and refined texture is probably unmatched in the entire animal fabric, and it receives for its nutrition a larger supply of blood than any of the other organs; its communications are the most extensive and varied, connecting it, more or less directly, with most other structures, by means of the nerves, which, at first view, might be taken to be prolongations of its substance; and, finally, its very position being regarded, it surmounts, as if crowning, the whole corporeal mechanism.

And yet, whatever demands upon our attention, on grounds purely physical, may justly be made by the brain, the psychical considerations, associated with this organ,

still more powerfully challenge regards on its behalf. Whatever be the actual facts of the case, it is certain that, to the unsophisticated understanding, the brain has ever seemed to exist as the special instrument of THOUGHT; and how much there is implied in that simple proposition, —the manifestation of the CONSCIOUS PRINCIPLE with all its attributes! We intuitively localize consciousness within the head, which forms a cavity known to be filled up by the brain; and the popular phraseology of all nations has used the terms, *head* and *brain*, to express and to characterize the capacity of thought. In our own language, it is said, in the description of a feeble-minded person, he has got *no head—no brains*, or, he is a *weak-headed* person; and, in an opposite sense, he has a *famous head*, or, he has a *cunning brain*. The poets and dramatists of every epoch and clime, employing the language of constant experience, frequently speak of the mind, or conscious principle, under the designations of head and brain. The infliction of injuries upon the brain is always observed to influence, in some way or another, the activity and power of thought; and, reciprocally, prolonged and anxious exertion of the mind sensibly reacts upon the contents of the cranial cavity.

Not only on these grounds does the brain possess the highest claims upon the labours of physiologists,—not only because of its refined anatomical structure, and because, in our experience of the present life, it has ever been noticed as the associate of the intelligence, but because, moreover, it is an organ absolutely essential to the prolonged continuance of life, and one, the healthful condition of which, most powerfully influences every other vital organ. Causes which affect injuriously the condition of the cerebral structure, deteriorate, in consequence, both the circulatory and digestive functions; and, in this way, deprave nutrition and diminish vigour. And thus, in whatever point of view this organ be regarded, it must rightly be deemed the

most important of the body, and its functions or offices among the highest objects of human study.

Surely, when proper consideration shall have been bestowed upon these simple and universally recognised truths, a second-rate position will no longer be assigned to the *cerebral* physiologist. Yet to him who elaborately investigates the functions of the lungs and viscera,—to him who astonishes by injection of the minuter tissues, and who, microscope in hand, strives successfully to shed some additional light upon the mode of interstitial deposition or absorption;—or to him who, by the novelty, and the boldness, and the cruelty of his vivisections, sheds at best but a doubtful lustre on some branch of physiology;—to such as these, most commonly, are contemporary honours awarded, and the first places conceded. "Strange to say," observes Dr Cowan,[1] "the discovery of the function of a single nervous filament, seems to excite more interest and controversy among the majority of medical observers, than the ascertaining the functions of the brain; and those who have devoted their lives and talents to what is incomparably the most important and difficult of all physiological problems, have too frequently only encountered ridicule or opposition, academic condemnation, and scholastic contempt. Our estimate of effort is not always proportioned to the value of the object which it seeks to attain, and our respect and admiration, for a time, are often withheld from those whose labours have most tended to advance the best interests of mankind. The period, however, never fails to arrive, sooner or later, when public opinion acknowledges the force of truth; and we believe a far higher rank will be conceded to those who have so successfully laboured to advance our cerebral physiology, than has hitherto been their good fortune to secure."

Yes, it is indeed true, that, associated with its functions,

[1] Prov. Med. Transactions, vol. vii. p. 401.

the brain has not received that amount of attention and investigation, which, *à priori*, was to have been expected. Although at every period wherein the study of anatomy and physiology has flourished, able men have constantly arisen who have laboured diligently and strenuously at this subject, it has yet, from several causes, failed most generally to interest the bulk of their disciples and students. The last century or two have witnessed a succession of eminent men, who have more or less devoted themselves to researches on the brain; there have been Sylvius, Willis, Vieussens, Sœmmering, the Wenzels, Rolando, Reil, Serres, Tiedemann, Flourens, Gall, Spurzheim, Vimont, and some others, all of whom have furnished us with some record of their labours; and yet how does it happen that they have failed to interest the masses, and that the field of their investigations has, in most instances, exhibited so scanty a harvest? The probable causes of this state of things are various; amongst the most prominent of these may be placed the supposed hopelessness of any inquiry concerning the relations subsisting between mind and matter. Physiologists have always observed the intimate connexion between the conscious principle and the cerebral organization; and, in some instances, general propositions have been advanced expressive of the dependence, in this life, of the one upon the other; yet, up to a recent period, there was always a want of definitiveness in every proposition hazarded, even with respect to the general fact, that the mind was associated with the brain as its organ. It has often been proclaimed that the brain is the organ of the mind, when those teaching and those receiving this doctrine, have not settled the idea to be attached to this latter term; by some, the purely intellectual faculties have been comprehended, and the affections, passions, and sentiments, have been excluded from the definition; by others, the exact meaning has been left uncertain; and only since the investigations of Gall, has the brain been, in any case,

fairly and unequivocally regarded as the instrument of manifestation of the conscious principle, with *all* its attributes,—feeling and intellect alike. Still the presumed impossibility of settling, decisively, such questions, has certainly indisposed many to the prosecution of cerebral physiology; thus, in one of the latest works wherein this topic is discussed, it is affirmed, that—" The nature of the connexion between the mind and nervous matter has ever been, and must continue to be, the deepest mystery in physiology; and they who study the laws of nature as ordinances of God, will regard it as one of those secrets of his councils 'which angels desire to look into.'"[1] To a certain extent, this may be true; the nature of the connexion between mind and matter, in its most intimate signification, will certainly be always hidden from us; but so are, and will ever be, the intimate causes of gravitation, electricity, chemical affinity, the movement of the sap in vegetables, and every other natural phenomenon. But this circumstance does not interfere with our search after the conditions under which these things occur, nor prevent us from obtaining large and legitimate inductions with respect to them. And so with cerebral physiology; we cannot hope to ascertain the essence of thought or feeling, or the ultimate essence or substance of the brain, nor even the precise mode in which structure manifests thought, or thought affects structure, because these are placed beyond the reach of our faculties. In speaking, therefore, of an analysis of the mind, or of cerebral matter, we exclude altogether these inquiries; yet, it is in our power, and within the lawful domain of human inquiry, to ascertain, in great measure, under what conditions of the brain particular manifestations of the mind occur; and in this way, from sound premises, we may deduce logical conclusions.

[1] The Physiological Anatomy and Physiology of Man. By Todd and Bowman. Part II. p. 262.

Heretofore, with certain exceptions to be subsequently dwelt upon, the physiology of the brain, even by its most prominent cultivators, has been detached from the study of mental philosophy; the mind, without any serious attempt at analysis, has been located in and associated with its presumed organ, and the *doctrine* has gone no farther. Like other departments of science, when improperly treated, it has, under such circumstances, yielded no practical results; and whether there has been question of training the mind in the healthful state, or of treating the brain when diseased and inducing mental aberration, precepts drawn from what is thus erroneously assumed to be physiological experience, have too often been found valueless, or conducive to mischief.

Yet an immense and unsurpassed amount of talent and industry has been bestowed upon this matter. Not only the eminent names just recounted have been deeply engaged in the investigation, but, as before observed, at all times when anatomical and physiological studies have interested philosophers, there have been found some who have devoted their best energies to the dissipation of the darkness encompassing the subject; and yet how is the barrenness of result to be explained? If great capabilities and corresponding industry have been applied in these researches, why should so little that is physiological have come down to us, universally recognized; and why, until very lately, should the most unmeaning anatomical descriptions have constituted almost the entire sum of our knowledge regarding the most exalted structure of the animal fabric? The reply to this inquiry may be preceded by another. From what cause has intellectual exertion, in any age, been devoid of useful result? How has it happened that, in the early history of any of the sciences, very often the ablest men, and the most laborious efforts, conduced to no progress? For the very obvious reasons, in these days well understood, that the talent was misapplied,—that the

labour was expended in a wrong direction—that the way was not rightly appreciated. The method of investigation being faulty, no sure or permanent addition to our knowledge, in any department, is ever to be hoped for. In the study of the relations subsisting between the structure and functions of the encephalon, as in any other scientific pursuit, more valuable results may be expected from men of plain understanding prosecuting a just mode of examination, than from those of the highest genius who do but pursue a devious track. In the language of Lord Bacon, " a cripple in the right way may beat a racer in the wrong one. Nay, the fleeter and better the racer is, who has once missed his way, the farther he leaves it behind."

Unbounded industry in determining the physical peculiarities of the brain ; a comparison of the human structure with animal brains of all species ; a watchful observation of the changes this organ undergoes in the progress of embryonic development ; its absolute and relative weight at all ages, and in all nations ; its most intimate constitution, as revealed by the microscope and by chemical analysis ; all these sources of knowledge have been appealed to, as if in expectation that function should be made known. The changes induced by disease, leaving their traces on the inanimate brain, have, with most commendable diligence, been watched and recorded for years, in the same anticipation. Living animals have been subjected to the severest sufferings ; their brains have been mutilated ; philosophers have " tortured nature," truly ; but, through this means, they have but rarely detected her secrets. Whatever advantages science may have indirectly derived from such researches, it will be the object of the present work to show that they have ever failed, and that they are essentially inadequate to develope the particular physiology of the brain.

In this state of things, the results that have been obtained by physiologists generally, have been few, unsatisfactory,

and contradictory among themselves. Whilst all have, in some way or another, recognised the brain as more particularly identified with the mind than any other organ, no precise or uniform meaning, as before observed, has been attached to the terms of the proposition. Some by *mind* have understood only the intelligence which they have located in the brain, and the feelings they have referred either to the viscera or to the ganglia of the great sympathetic system of nerves; others have assigned to the encephalon the manifestation of all conscious attributes, referring the intelligence to the cerebral hemispheres, and the instincts and emotions to certain ganglionic masses of *cineritious* matter (so called) at the base. In every statement, however, including views of this kind, nothing is more striking than the complete absence of all exactness of expression. Mention is made of intellect, reason, power of mind, intelligence, instincts, emotions, and so on; but the usages of speech attach no fixed, particular, or definite idea to such terms; it is like speaking of *air*, instead of oxygen, hydrogen, or any other specific gas. Intellect may mean the *memory* or the *judgment*, each being, in the estimation of mental philosophers, distinct qualities of the mind, equally so as any two of the external senses. Reason, intelligence, instinct, emotions, as terms of scientific value, have scarcely any precise signification. Mankind are not altogether agreed as to what "reason" is, nor whether it inheres exclusively in our own species, or also in some of the higher animals. "Intelligence" may, in some sense, apply to the lowest conscious being as correctly as to the highest, every animal being *intelligent* within the sphere of its proper conscious activity. And so with respect to "instinct" and "emotions," what are they? Sometimes modes of actions, rather than special powers; and, if so, they cannot be expected to have distinct seats within the organism, any more than can *whispering* be supposed to be effected by one vocal apparatus, and *loud speaking* by

another. Fear, when sudden and intense, is experienced *emotionally;* but it is recognised in a different manner when it moderately and continuously operates, inducing circumspection. To pursue this theme, however, would be to anticipate; the intention is simply to illustrate the indefinite character of the mental philosophy developed by the physiology of those investigators whose conclusions have been arrived at by an erroneous route.

Although the difficulties of the subject, as displayed in the imperfect results obtained by so many men of science, may not have deterred master spirits from returning to such researches, it is yet certain, that the absence of all uniformity, alike in the inferences and the data from which they have been drawn, has contributed greatly to discourage large numbers from any attempt to acquire some positive knowledge concerning the physiology of the brain, and it has often been abandoned as a field of inquiry that was sterile and unproductive. The various teachings on the subject have much more frequently been regarded as amusing hypotheses, or as idle speculations, than as containing solid information, capable of being applied in the practical departments of life. Thus, for the most part, it has happened that, when cerebral physiology has necessarily claimed, in books or in lectures, some share of attention, it has either been summarily disposed of by vague references to the brain's connexion with certain nerves, and to its presiding influence over the nervous system generally,—or certain current ideas, received from the teacher's immediate predecessor, have been repeated,—commingled, possibly, here and there with some favourite speculation of his own; and so, among certain classes, the *physiology* of the brain has been at a dead stand, notwithstanding the prodigious acquisitions to our knowledge of its *anatomy*, both in health and disease. The history of every science has exhibited some such state of things, as, under like circumstances, will ever be the case; until observation and experiment,

rightly conducted, supersede speculation and inference from vague analogy, the mind of the student will always be "imaginary parallels, correspondencies, and relations, which have no existence."

And yet there is no good reason why an inquiry into the functions of the brain, and of its various parts, should not be successfully prosecuted; for there is no obscurity in this department of physiological research which is not common to many others. Cerebral physiology has its own obstacles and its own difficulties, like any other branch of science, requiring certain mental peculiarities and favourable opportunities for their removal and dissipation; but still, under just circumstances, the discovery, not only of the general office of the brain, but of the functions of its particular parts, is an object of pursuit for the philosopher as legitimate, and as likely to reward the labour of investigation, as the solution of any other physiological problem. The attempt will be found useless, or productive of imperfect or contradictory results, only so long as the method pursued is defective or faulty. All sciences,—physiological not less than those purely physical,—have sustained both error in their development and retardation in their progress, wherever they have been cultivated, without the guidance of sound principles in the investigation. Under such circumstances, talent has but led to confusion, and industry has yielded no fruit. How long were mankind before the true relations of the heart and blood-vessels to the circulating fluid were satisfactorily explained; and yet the physiology of the blood, and the nutrition of the system, had always been objects of interest and attention. For ages, physicians and philosophers received and taught the erroneous doctrine that a vital air proceeding from the lungs to the heart, passes through the aorta, and is distributed by its branches throughout the body, in this way giving rise to the arterial pulsation; no account being taken, from ignorance of the fact, of the circulation

of the blood. Such a conclusion resulted from no want of mental acumen on the part of our predecessors, but from fault in their method of inquiry. Cesalpinus and Servetus, pursuing, in the sixteenth century, a better, but yet imperfect mode of research, obtained some glimpse of the truth. And when Harvey applied to the investigation the direct and sure method of induction, by carefully demonstrating the function, and comparing this with the aptitude of structure, and appealing to other sources of information for the confirmation of his positions, a sure addition was made to our knowledge, and the physiology of the heart and the blood-vessels was established on a scientific basis. Again, take the physiology of the spinal cord. Up to a recent period, it was for the most part regarded but as a continuation of the cerebral structure with the same *general* functions; the conclusions in this respect hardly assuming to rest upon any particular facts. Although Galen had conjectured that there were nerves for three distinct purposes; namely, for sensation, for motion, and for the discrimination of what may be salutary, and what injurious to the system; making, at the same time, a distinction between the powers of voluntary and involuntary motion; it was not until speciality in function was studied in connexion with divisions of the anatomical structure, that any approach was made to positive knowledge on this matter; and it was only after a sound mode of inquiry had taken the place of one that was essentially vicious, that we had any definite physiology of the spinal cord. To this subject, however, more detailed reference will be made in the sequel; and the illustration need not be here extended.

The mode in which the functions of the brain should be ascertained is by no means settled among physiologists. Canons for our guidance in this respect that are of universal recognition, do not exist. And, yet, at a time when science is achieving its mightiest triumphs, and when, in this great movement, the brain and nervous system are beginning to

receive a fair share of general attention and study, and when, by the acknowledgment of most physiologists, so much obscurity continues to envelope cerebral physiology, it is surely time that this problem should be solved, and the question settled : How are the offices of particular parts of the brain to be determined? What method of investigation is best calculated to develope the relations subsisting between the structure and functions of the encephalon?

Desmoulins formally propounds three modes of becoming acquainted with the physiology of the brain and nervous system, and he is followed in his philosophy by large numbers of modern inquirers. "The first is that of experiment: by removing successively the several parts of the brain and its appendages, and by observing what effect is produced by these successive removals, we attempt to gain the knowledge of the specific uses, both of the parts that are removed, and of those that are left. The two other modes proceed upon the principle of induction. They consist in duly appreciating the facts which are to be obtained by the study of comparative anatomy and of pathology. There is scarcely any part of the nervous system which is not wanting in some class of animals, so that by sufficiently multiplying our observations, we have the means of discovering the result of every combination of the cerebral organs, with respect to the powers and functions of the system. The symptoms and phenomena of disease afford us the same kind of inductive evidence, for the operation of the several parts of the nervous system, although seldom in that clear and decided manner as in the former case."[1]

In the present treatise, it is proposed to examine the validity of these three methods for determining the functions of the brain—vivisection, comparative anatomy, and pathology. The author believes himself to be in a condi-

[1] Bostock's Physiology, vol. iii. p. 386.

tion to show that each of these three methods, as leading to the primary fundamental evidence relative to the cerebral functions, is both defective and vicious; defective, by supplying insufficient data at the best; and vicious, by, in many instances, suggesting erroneous conclusions. It will not be denied that results, capable of being turned to useful account, may sometimes be educed by such methods of investigation; on the contrary, such results may have given, and will probably continue to give, many hints useful in the further prosecution of inquiry into the cerebral offices; and although they have hardly ever of themselves furnished any addition to our sure knowledge of the physiology of the brain, it is yet certain that their usefulness has often been witnessed, in the confirming and the strengthening, and in *trying*, as it were, the results obtained in another and more direct manner, and, in this way, furnishing secondary or indirect proofs. It will be the author's aim to show that, with respect to many of the published facts of vivisection, comparative anatomy, and pathology, the greatest uncertainty, and even contradictions, abound; that, the facts themselves being admitted, no agreement exists in regard to their doctrinal value, being conceived by certain physiologists to lead to some inference which others of equal authority controvert and deny; themselves, probably, deducing one that is totally different. It will appear, moreover, that almost every physiologist of weight, when untrammeled by the defence of some position which rests mainly upon such objectionable grounds, has readily conceded that the means in question are utterly inadequate to their proposed end.

After reviewing the character, history, and results of those methods of investigation which, it is here contended, are faulty and erroneous, the author will proceed to submit what he, along with many able physiologists, deems to to be the true plan of prosecuting cerebral physiology. Structure associated with function—magnitude in the

development of the former, in connexion with excessive manifestation of the latter—the size of certain parts of the brain, in alliance with corresponding powers of the mind—he believes to be the primary objects of inquiry. That the brain is the organ of the conscious principle, all physiologists of admitted authority are agreed; that, in the case of the nervous system at large, power and development usually coincide, is a proposition concerning which a like general consent obtains; and it is conceded to be a probable fact, that various portions of the encephalic mass have different functions. In the sequel, the writer expects to show that the offices of these particular parts can be made out only by noticing the invariable co-existence, *cæteris paribus*, of peculiarity in their development, and speciality in the mental characteristics; and that, when conclusions of this kind have once been directly gained, they never fail to harmonise with the sure results obtained from every other source; receiving from the phenomena of vivisection, comparative anatomy, and pathology, a further confirmation; and throwing back upon them a light which greatly assists in the interpretation of such phenomena, as matters previously obscure.

CHAPTER II.

ON MUTILATIONS OF THE LIVING BRAIN AS A MEANS OF DISCOVERY IN CEREBRAL PHYSIOLOGY.

The impossibility of gaining additions to our knowledge of the cerebral functions by mere dissection of structure, would appear to be generally conceded. No examination of the intimate constitution of the dead brain could ever do more than exhibit its physical qualities. The most powerful microscope could but reveal its molecular disposition. Nevertheless, a minute anatomical investigation of parts, the functions of which have already been determined, may shed considerable light on the mode in which these are performed; and the sympathies subsisting between one animal function and another, are sometimes beautifully illustrated, by demonstration of a correspondence in the organic arrangements. This, however, always presupposes that function is ascertained. In this state of things, minute anatomical researches would naturally succeed, rather than precede, the recognition of specific offices in various parts; yet when the brain is concerned, the contrary rule is too often acted upon. Whilst by many physiologists no sure information is admitted respecting the precise function of its various parts, inquiries, conducted by aid of the microscope and by chemical analysis supply to us results affecting its physical characteristics of so varied and complex a kind, that, without a very arduous study, a clear comprehension of the details is almost impracticable. One really wonders what many of these investigators propose

to themselves; they cannot expect that, in this way, a revelation of function will take place; and yet without such an issue, no application of the facts to physiology can be made; nor, otherwise, can any beneficial influence upon practical medicine be exerted. It may fairly be questioned, whether it be not the fault of the present day to give undue importance to mere facts without regard to general principles, just as philosophers in foregoing ages erred in the other direction,—by disregarding the accumulation of experience in their premature and hasty generalisations. "Disgusted," says Dr A. Combe,[1] "with the visionary theories which once maintained a mischievous ascendancy over the minds of men, and led them far from the observation of the phenomena occurring in the great laboratories of nature, we have passed, in our aversion, almost to the opposite extreme, and, discarding general views, we cry aloud for *facts*. And as facts are the only basis of accurate knowledge, it is fortunate for mankind that the present mode should be attended with so much practical usefulness. But facts alone are not sufficient; and unless they be collated, and their relations to each other and to general laws be deduced by a careful induction, they lose the greater part of their value, and become, to use the apposite illustration of an able writer on political economy, little better than the undigested erudition of an almanack-maker, and afford no means of judging of the truth or falsehood of a principle or rule of practice."

The sterile results of mere anatomical researches, would seem to have suggested the mutilation of the living organism as a means of discovery in cerebral physiology; and certain conclusions which some of the earlier vivisectors arrived at in this way having been confirmed by subsequent experience, an importance has become attached to this method of investigation which it but little merits, and

[1] Observations on Mental Derangement, Introduction, p. xxiv.

which it never would have obtained, had its whole history been duly considered. Many of our minute, laborious, and pains-taking anatomists, when they do arise from the dryness of mere detail, and deign to glance at the *physiology* of the brain, generally include the inferences of vivisectors as among the nearest approaches to ascertained truth; yes, even in those instances where the whole procedure, abstractedly considered, shall have received from them an unmistakeable condemnation, as regards its fitness for developing our knowledge of function.

If, "by removing successively the several parts of the brain and its appendages, and by observing what effect is produced by these successive removals," we could really "gain the knowledge of the specific uses, both of the parts that are removed, and of those that are left," any objection to this mode of experiment, resting on its mere cruelty, might justly be overruled. But is it quite so certain that this proceeding is at all adapted to the attainment of any such end? Let us examine this question in detail.

Before the expected result could reasonably be looked for, many conditions are absolutely demanded, an absence of any one of which would vitiate, or leave defective, the entire affair. In the first place, the instincts and powers, as displayed in the habits of animals made the subject of operation, must be recognised systematically; no mere general acquaintance with these could suffice; unless they were fairly defined and systematised, how could we determine the absence of one or more, and the presence of the remainder, in any given case? The mental philosophy, so to speak, of each animal selected for cerebral vivisection must, before experiment, be agreed upon. Will the most profound natural historian assume, for a moment, that this is possible, in the existing state of our knowledge? Certainly not; and so the very first prerequisite of success is wanting to this method of investigation. Further, if it were conceded that the above conditions could be realised,

it would be necessary that the animal, whose brain had been mutilated, should maintain an existence of at least some weeks after the operation; for, otherwise, sufficient opportunity could not be afforded for watching the result, seeing that scope for the performance of the whole series of its instinctive and intelligential actions must be allowed, or the absence of none could be assured. We know, however, that only in a few instances, and these somewhat problematical, and low down in the animal scale, would life persist, for any length of time, after serious lesions of the cerebral structure. Still, we will suppose that even this difficulty were surmounted. After a prolonged and almost uninterrupted observation of the mutilated animals' habits and movements, and on the exhibition of some undoubted defect, it would yet be uncertain how far this was occasioned by the loss of some one faculty coincidently with removal of its organ, or by disorder of several faculties from injury to a group of organs. If it were postulated that the brain is no congeries of organs, but simple in function, and manifesting the aggregate of faculties, vivisection of parts could supply no possible evidence of the fact that was new. In all experiments, moreover, upon animals, it must be remembered that they are in a position different to that of human beings; they cannot tell us what they inwardly experience; we can only judge of results by manifestation of muscular action; and how many sources of fallacy lurk here? One observer will attach a signification to a series of animal efforts after operation, which will be interpreted in quite a contrary sense by some other. In the sequel, it shall be shown that, in point of fact, such has always been the case; no two vivisectors, conducting their proceedings independently, hardly ever coinciding in the statements which they make regarding the consequences of the same mutilation.

A group of difficulties, however, of another order, oppose themselves to success in the results of such experiments.

In any attempt, by vivisection, to determine the functions of portions of the encephalic mass, there ought certainly to be a practicability of removing, or destroying, one division without causing sensible lesion to any of the others; this must be admitted; or, if it be denied, what value should attach to conclusions respecting the functions of any part, when others participate in the injury? But, what happens, indeed, in these vivisections of the encephalon? First of all, the integuments investing the cranium are wounded; here at once, we have a probable cause of muscular exertion in the animal to complicate the coming results. Then, the cerebral membranes are injured; can this take place without some irritation and extravasation occurring? Last of all, the substance of the brain itself must be cut and scooped; and what prodigious laceration of fibre, and infiltration of blood and serum must thence of necessity ensue. Yet, we are to be told that, after such a proceeding, we may "gain the knowledge of the specific uses both of the parts that are removed, and of those that are left," by observing "what effect is produced by these successive removals."

Let us take into account the manifold offices of the entire nervous system in the higher classes of animals on which we are supposed to be operating; let consideration, also, rest upon the extensive sympathies subsisting between all its parts; and then, if we can, let us say what worth there can be in these mutilations of the living brain. It is known, and admitted by all, that every division of the nervous system holds some communication with the others; that the fibres of one part are continuous, and interlace with those of others, whose functions are separate and distinct; that under such circumstances, an affection of some particular set of fibres may not only lead to depravation of their function, but of those with which they communicate, even to the remotest parts of the system. This happens occasionally in tetanus. A few fibres at the ex-

tremity of a finger sustain laceration ; an irritation extends along their whole course, to the spinal origins ; hereupon, the cord participates in the injury ; almost the entire nervous system becomes, more or less, involved in the lesion ; the frame is shaken, and convulsed by the morbid influence ; and, often, the patient dies. Other remarkable, yet more simple, examples of nervous sympathy are familiar to every medical practitioner. A decayed tooth frequently induces far more disturbance in the facial nerves, than in those of its own immediate locality ; tic doloureux, arising from a bad tooth, is a circumstance common enough. Disease of the hip-joint, in the incipient stage, makes itself felt in the nerves about the knee, rather than in those of the hip-joint itself. A renal calculus occasions a dragging sensation along the back of the thigh, in the course of the sciatic nerve ; and stone in the bladder gives rise to benumbing sensations at some distance from its actual situation. Indeed, the instances might be adduced in abundance, so familiar are they to all who are engaged in the treatment of disease. There are sympathies which arise from appreciable communication of nervous fibre ; there are others which appear to be dependent upon some association in function, subsisting between different organs, as the uterus and the mamma ; and others apparently originate in the mere contiguity of parts, as in the case of the bladder and the rectum, where there is no special connexion, vascular or nervous. Now, if we reflect upon these various conditions of sympathy, it will be noticed that, among the various parts of the encephalic mass, they all prevail in an eminent degree. There is intimate communication and interlacement of cerebral fibre ; there is perpetual association of function ; and the contiguity is complete. Under all these circumstances, it surely is not by mutilations of the living brain, that we can establish an acquaintance with its physiology ; that we can ever "assign to each part of it its specific office."

Let us here examine the actual facts of the case. By tracing some little account, historically, of these encephalic vivisections, we shall attain a still firmer position in pronouncing finally with respect to the inadequacy of the method now under discussion. From the subjoined statements, it will be apparent that the facts themselves, when they have been obtained by different experimenters, never present the same aspect; that, indeed, they are positively contradictory one of another; and that, in this way, they become deprived of all the value they might otherwise possess. As the cerebellum has been the favourite subject of vivisection,—presenting as it does the greatest facilities for operation, owing to its comparative detachment,—the mutilations which have been practised upon this structure, shall mainly come under review; and if their value be disproved, no importance can be attached to other lesions of the encephalon, for very obvious reasons.

Rolando, of Turin, in the early part of the present century, having, from consideration of the anatomy, been led to regard the cerebellum as in some way or another concerned in locomotion, proceeded to test the validity of his opinion by the following experiments.[1] He removed, by successive efforts, as much of the cerebellum as he could from one side of a pig, and also of a sheep. But scarcely did the lesion extend beyond the trepanned side, than the animal *was struck with hemiplegia*, and it perished very soon *amidst convulsive spasms* and hæmorrhage. He cut the cerebellum, in one of the animals, in different ways; the animal *could no longer sustain itself upon the legs*, as if it were paralytic; after twenty-four hours, it died of convulsions. Rolando professes constantly to have observed, that a diminution of the movements was in a direct ratio

[1] The account is detailed in a work by Rolando, on the Brain and Nervous System, published in 1809. The statements in the text are taken from Gall's "Sur les Fonctions du Cerveau," vol. vi., which contains extracts from a French translation of Rolando's work by M. Coster.

with lesion of the cerebellum; the animal being sometimes paralytic altogether, at times only hemiplegic, according as the cerebellum was destroyed in whole or in part.

The experiments were extended to birds. Rolando removed nearly the half of the right side of the cerebellum of the common domestic fowl; immediately *it became paralytic*, and fell on the corresponding side, without in any manner being able to make use of the right leg, or execute with this leg the least movement. Finally, the *paralysis extended itself to the two sides.* It is acknowledged, however, that this fowl occasionally shook its wings, and even moved its inferior extremity,—a result attributed either to the mere mobility of the muscular fibre, or to some remnant of the cerebellum.

Early in the year 1822, a report, drawn up by the late M. Cuvier, was presented to the Royal Academy of Sciences of the Institute, on a memoir by M. Flourens, having for its object the determination of the properties of the nervous system, and of the action of these, and of different parts of this system, in the motions called voluntary, or those of locomotion and prehension. The following extract relates to cutting and removal of the cerebellum, and is taken from a translation of M. Cuvier's report, as it occurs in Mr Solly's valuable work on the brain. The subject of experiment was a pigeon.

" During the ablation of the first slices, only a little weakness and a *want of harmony in the movements* occur. At the removal of the middle slices, an almost *general agitation is the result.* The animal, continuing to hear and to see, only *executes abrupt and disorderly movements.* Its facilities of flying, walking, standing up, &c., are lost by degrees. When the cerebellum is removed, the *faculty of performing regulated movements has entirely disappeared.* Placed on its back, the creature could not get up; yet it saw the blow that threatened it; it heard noises; it endeavoured to avoid danger, and made many efforts to do so, without accom-

plishing its object. In a few words, it retained the faculties of perception and of volition, but it had lost the power of making the muscles obey its will. It was with difficulty that the bird stood up, resting upon its wings and tail. Deprived of its brain, it was in a dormant state; deprived of its cerebellum, it was in a state of apparent drunkenness."[1]

M. Bouilland repeated these experiments, and published an account of his proceedings in a pamphlet, in which the results are stated in the following terms :—" Mutilations of the cerebellum *were not accompanied by paralysis*, or convulsions, properly so called, but merely by *disorder of the locomotive functions;* the *faculties of equilibrium and progression were destroyed.* The animals mutilated were still capable of reflection, of hearing, of *moving their limbs in all directions*, and most frequently these movements were executed with extraordinary quickness and violence."

* * * When the cerebellum is totally disorganised, or entirely removed, the animal is for ever deprived of the faculty of equilibration, of walking, and of flying, if a bird : all the efforts it makes are useless ; they merely demonstrate, that though unable to perform any combined motions, out of which station or locomotion results, it nevertheless retains the faculty of executing partial movements, and of moving its limbs in all directions.[2]

Magendie, who practised also these mutilations, states that, if a wound be inflicted on the cerebellum, the animal seems *compelled by an inward force to a retrograde movement*, although making an effort to advance ; and that if the *crus cerebelli* on one side be injured, the animal is caused to roll over towards the same side. Sometimes the animals made sixty revolutions in a minute, and continued this movement for a week without cessation. Division of the second *crus cerebelli* restored the equilibrium.[3]

Magendie, moreover, states, in his work on Physiology,

[1] Solly on the Brain, p. 306. [2] Ibid. p. 314.
[3] Vide Carpenter's Human Physiology, 2d edit. p. 217.

that he has seen, and has demonstrated to others a great many times, in his course of lectures, animals deprived of cerebellum, and which, nevertheless, *executed very regular movements.*[1]

The following quotation furnishes an account of further experiments on the living cerebellum, practised by M. Fodera, in presence of Gall, Dannecy, Fossati, Londe, and Georget; the results are given by Gall, in the subjoined statement:—

"The two first rabbits from which the central and superior parts of the cerebellum were removed, died in three or four minutes. The hæmorrhage, as well as the *convulsions*, were considerable. The section was made from below, upwards. In one of them, the *medulla oblongata* was reached by the instrument, and tetanic convulsions were the results."

"The same operation was performed on a third rabbit. It had an astonished look, started, ran almost constantly backwards, drew the head several times backward, leaped into the air in starting; twice it leaped to the height of a foot and a half. When it was pricked, it drew back its limb rapidly. We destroyed, by little and little, the whole of the centre, and a portion of the hemispheres of the *cerebellum*. The symptoms increased with the destruction."

"The first pigeon whose cerebellum we destroyed, like the preceding rabbit, drew its head strongly backwards, *going sometimes forward* and sometimes backward."[2]

Here, then, we see the results of mutilating that particular division of the encephalon, which furnishes the greatest facilities of all for its separate lesion. The varying and contradictory character of the effects obtained, will be appreciated at a glance by referring to the sentences in the foregoing extracts which have here been rendered in italics. Rolando found the vivisection in question to induce

[1] Milligan's Translation, 4th edit. p. 183.

[2] Combe's Translation of Gall on the Cerebellum, p. 405.

convulsions and paralysis; Flourens gained by his proceedings a loss of harmony in the action of voluntary muscles; Bouillaud's subjects sustained no paralysis, but a destruction only of the powers of maintaining equilibrium and of progression, with retention of muscular mobility in any direction; Magendie obtained an invincible disposition to retrograde movement, stating that his animals, after loss of the cerebellum, could yet execute very regular, combined movements; and, lastly, Fodera's mutilations were followed in some instances by convulsions and death, in others, by kicking and leaping, and by movements *progressive* as well as retrograde.

It will thus be seen that no two of the above instances presented anything like coincidence in the results; but that, on the contrary, direct contradictions occur. Rolando's paralysis is met by Bouillaud's no paralysis; Flourens' *inability* to *regulate* movement, is counterpoised by Magendie's *capability*, confirmed by Fodera's experience; and the same contradiction is seen throughout the entire history of these vivisections; there is no single fact recorded by one operator, which is not counteracted in its tendency to any conclusion, by the experience of some of the others.

Diverse as are the statements regarding the visible effects of lesions of the living cerebellum, the inferences regarding the functions of this organ deduced by the experimenters themselves vary quite to the same extent. Rolando proclaimed it to be the source of locomotive power; Flourens decided that it was no such thing, but only the *co-ordinator* of the voluntary movements, and the *balancer;* Bouillaud maintained that it balanced the body only in station and progression; and Magendie concluded its office to be the source merely of movements forward. Gall, who carefully watched the repetition of these experiments, drew no positive inference from the results which he witnessed, observing, that "they will always be found to differ according to the irritability and the age of the creature, and

according as the instrument is blunt or sharp, so that it pulls more than it cuts."

Many mutilations of the *cerebrum* have been practised, with equal or greater contrariety of consequent doctrine; those performed on the cerebellum, however, have alone been detailed, for the reasons before stated, and also because from many individuals they have received an amount of attention and consideration which, it is conceived, can have arisen only from a total disregard, or forgetfulness, of their true history.

The experiments of Flourens, in particular, are perpetually appealed to, as if they furnished results uniformly to be obtained; whilst those of the other vivisectors receive but little notice, and that only in so far as they are supposed to coincide with those of Flourens. The great name of Cuvier, always mixed up with them, has probably supplied the reason of this pre-eminence. It is yet remarkable that physiologists, commenting on mutilations of the cerebellum, and assuming the effects narrated in Cuvier's report, to constitute the normal consequences, so to speak, do but rarely agree with Flourens as to their doctrinal value; scarcely any two deducing identical conclusions from the same premises.

M. Foville,[1] admitting the facts adduced, thinks they confirm his own notion, that the cerebellum is the central organ of sensibility. "The opinion advanced," says he, "that the cerebellum is the regulator of the voluntary movements, if we attentively consider the reasoning on which it rests, seems to me to strengthen the idea which places the central seat of sensibility in the cerebellum. After having injured the structure of the cerebellum extensively, we have observed that auimals preserved the power of moving their limbs, but had lost that of co-ordi-

[1] Diction. de Med. Pratique, Art. ENCEPHALE, p. 204. Quoted by Solly.

nating the movements of these in a manner convenient to station, progression, flight, &c. But when we *will* to perform, and actually perform, certain movements, do we not distinctly feel that we execute them? The man who, with his eyes shut, moves his hand or his arm, does he not also as distinctly feel that he moves these parts as if he followed them with his eyes? whilst the paralysed man who, with his eyes shut, is desired to move the paralysed limbs, may be very willing to do so, though incapable, and perfectly aware of his incapability of obeying; nor would it be possible to persuade the individual so circumstanced that he did move his limbs."

"If this be true (and no one, I think, will doubt it), how can we expect, that an animal deprived of the faculty of perceiving the sensation of the movements which it executes, should execute them in the *ensemble* with harmony, and in accordance with a proposed end. How can we expect it to walk deliberately, and to keep its equilibrium, if it does not feel the ground upon which it stands, if it is ignorant of the position in which its limbs are placed? I remember conversing with Sir Astley Cooper on this subject towards the end of the year 1830. Sir Astley cited to me the case of a man completely deprived of the faculty of sensation in one arm and hand, the muscular power of which was however preserved. When this man was desired to take hold of and to lift any thing, he did so very well; but if, whilst holding the object, his attention was taken away from the hand, irregular contractions of the limb commenced, and very soon the object fell to the ground; as soon as the patient ceased to follow the contraction of his fingers with his eyes, nothing remained to inform him that he held the object, when, of course, it escaped from his grasp."

"This and other cases of a similar description seem conclusive as to the fact of sensation being the true regulator of the muscular motions; it is by means of sensation that

we are aware of the mode or degree of action of our muscles, that we have the power of co-ordinating their contractions in a suitable manner, and of executing a succession of voluntary movements in harmony with one another. The faculty of perceiving the movements being lost, we cannot answer for their precision or duration."

Such is the reasoning which Foville employs, in disproof of the essential office which Flourens and his followers would attach to the cerebellum, as revealed by its mutilations. It is hereby seen that, whilst Foville treats the results as if they were invariable, and matters of course (which it has just been shown that they are not), he yet regards the inferences to be unwarranted by the facts of the case.

Dr Bostock states that Desmoulins disagrees with both Flourens and Rolando, in thinking "that the cerebellum is the great agent either in producing or regulating muscular motion, an hypothesis which appears to be disproved by the most direct experiments."[1] Dr Bostock himself says, in reference to the experiments in question, "In some cases I should be inclined to draw an inference precisely the reverse of that which has been formed by the author."[2]

Dr Alison, suggesting his own explanation, remarks, with regard to the effects induced by the mutilation, "This may be supposed to be, either because the injury produces certain permanent uneasy feelings, such as vertigo, which interfere with and confuse the sensations, by which the voluntary movements are regulated; or because the recollection of muscular sensations, which are the guide to all definite voluntary movements, depends upon the cerebellum, and is lost when it is destroyed."[3]

Mr Herbert Mayo, on the same subject, says, "The simplest explanation of the phenomena above described, is to suppose that an injury of the cerebellum, to a certain

[1] Bostock's Physiology, vol. iii. p. 390.

[2] Bostock's Physiology, vol. iii. p. 374. [3] Alison's Physiology, p 253.

depth, produces a sensation analogous to vertigo; that the animal conceives itself either to be hurried forward, and makes a more or less perfect exertion to repel the imaginary force, or to be moving backward, and moves its limbs to a certain degree in correspondence."[1]

Mr Solly in discussing the present subject, observes, "A candid consideration of M. Foville's opinions must, at any rate, convince us that the views of Messrs Flourens and Bouillaud cannot yet be regarded as established doctrines of physiology."[2]

Thus, then, it would appear that, under circumstances the most favourable for encephalic vivisection (which the instance of the cerebellum certainly supplies), no uniformity in the results exists; that the facts proclaimed by different physiologists are opposed one to another; and that the inferences deduced are as various and contradictory as the premises. This is so obvious, that had not all of these writers had a prejudice against Gall's views, they would, probably, never have attached that importance to these experiments, which most of them have done. The chief merit of them in their eyes seems to have been, that they appeared to afford something like fact and argument in opposition to Gall.

It will, however, be asserted that vivisections have, in many cases, advanced the progress of physiology, and especially that, in the determination of the functions of individual nerves, the method has proved itself to be sound and effective. That the experimental proceedings with living animals have been turned to useful and practical account, is certain; but what fact, noticed in the animal economy, under any set of circumstances, will not be beneficially employed by a philosophic observer? This, however, is not the place to discuss the true value of vivisections in all cases; the present purpose is to determine

[1] Mayo's Physiology, p. 245. [2] Solly on the Brain, p. 323.

their worth *as a means of discovery in cerebral physiology;* at the same time, the writer conceives himself to be authorised in affirming that, even in the case of particular nerves, the functions have not, properly speaking, been made out by vivisections; and that, where any certainty exists on the subject, a great deal had been done to bring this about before the mutilations were practised; and that these latter may be said to have confirmed what was previously all but ascertained, rather than to have constituted the primary means of discovery, after the manner of modern attempts by lesions of the living brain. This subject, however, in its positive bearings, will receive a more ample discussion in a subsequent part of this treatise. At present, it may be sufficient, in going back to the history of experiments on the nerves, to show that, if the experimenters had commenced in utter ignorance of the definite functions of various parts of the nervous system,—if they had proceeded, in the absence of all preliminary and fruitful research, to have cut, and simply to have looked at the immediate consequence, the results would have been little less jejune than those with regard to the cerebellum, and other portions of the encephalon. And as the discovery of the functions of the spinal nerves, and the various mutilations practised in the course of the investigations, have contributed more, probably, to mislead physiologists, in their estimate of the actual value of vivisections, than any thing else, it shall here be shown how little uniformity, either in the facts or the conclusions, has been obtained in the mere matter of the mutilations; and of what little valuc the inferences would have been, had they been deduced solely, or primarily, from noticing "what effect is produced."

When Sir Charles Bell experimented on the facial branch of the fifth nerve, he found, on dividing its trunk, that the power of elevating and projecting the lip, as in gathering food, was no longer exerted; and thence inferred that its

function was for voluntary motion as well as for sensation. Other experimenters showed that here was a mistake, and that the motion did not occur, because of the absence of excitor sensibility; just as Foville reasons on the defective movements after destruction of the cerebellum. Whilst Bell is considered to have really established the separate functions of the different roots of the spinal nerves, the anterior being for motion and the posterior for sensation, the history of this question presents the greatest contrariety in the results of vivisection. Bellingeri was in this way led to the belief that the anterior roots were for the flexion of the various articulations, and the posterior for their extension. Magendie, in prosecuting this department of experimental inquiry, maintained that the anterior were subservient in some respects to sensation as well as to voluntary motion. Altogether, the effects consequent upon the vivisections were any thing but decisive; and it was only by the correction and explanation which they received from other sources of knowledge, that conclusions at all satisfactory could be drawn from them. It is not meant that there is a precise parallel between such experiments, and those that have been referred to in the case of the encephalon; the conditions of experiment vary most materially. In a subsequent part of this work there will be occasion to refer to the matter in this point of view; they have been noticed at present as indicating that, even in the *chef d'œuvre* of the vivisectors, the course has not been quite so smooth, and so facile, as popularly supposed. Further, it may be observed that, up to a very recent period, the vivisections in question were always conceived to determine that the functions of divisions of the spinal cord were respectively similar to those of the roots connected with them. "No one," says Sir Charles Bell,[1] "has given reason to doubt the correctness of the statement that I

[1] Bell's Anatomy, vol. ii. p. 388.

have made, that the anterior column is for motion, and the posterior for sensibility." How prematurely all this was assumed, will be apparent from the following circumstances related by Dr Carpenter, who states as follows:—" The recent experiments of Valentin, whilst they fully confirm Sir C. Bell's determination of the functions of the roots of the nerves, coincide, to no small degree, with Bellingeri's opinion in regard to the offices of the anterior and posterior divisions of the cord. He obtained reason to believe that, in the frog, neither the superior nor inferior strand of the cord (posterior and anterior columns in man) solely possesses motor functions; but he found that, when the former were irritated, sensations predominated; and when the latter, motions were chiefly excited. He further states that, if the superior strand (posterior column) be irritated at the point at which the nerves of either extremity are given off, that extremity is *extended*; and that if the inferior strand (anterior column) be irritated, the extremity is *flexed*. At their entrance into the spinal cord, therefore, it would appear that the motor fibres of the extensors pass towards the superior stratum (posterior column in man), whilst those of the flexors are continuous with the inferior stratum (anterior column); their course being more altered, however, when they are examined far from the point of issue."[1] And, in the latest publication of any standing which the author has seen, and which refers to this subject, the following occurs:—" Could it be proved that the anterior or motor roots were exclusively connected with the antero-lateral columns, and that the posterior or sensitive ones arose exclusively from the posterior columns, then there would be good anatomical grounds for the doctrine so long erroneously prevalent, that the functions of the columns coincided with those of the roots, that the posterior columns were sensitive, and the anterior motor; but no-

[1] Human Physiology, 2d edit. p. 129.

thing is more certain than that both roots are connected with the antero-lateral columns. Hence, all that anatomy warrants us in stating is, that the antero-lateral columns are probably compound in function, both motor and sensitive. Respecting the office of the posterior columns little can be said."[1]

Other mutilations, exploratory of the functions of individual nerves, have produced opinions and views equally discordant. Thus, with regard to the sense of taste: that it constitutes a *special* sense, like smell, sight, and hearing, physiologists are now pretty well agreed. In the uncertainty which has existed as to its subservient nerve, the vivisectors have again taken the field; with what uniformity in the results, let the following facts decide. In 1834 or 1835, Professor Panizza cut sundry nerves supplying the tongue, and drew the conclusion from the effects produced, that the glosso-pharyngeal was the special nerve of taste; immediately after the publication of these experiments in this country, they were repeated by Mr Herbert Mayo, who obtained contrary results, and maintained that, not the glosso-pharyngeal, but the lingual branch of the fifth nerve, endowed the tongue with gustatory sensibility. Shortly after, Dr Alcock of Dublin, followed in the same track, and concluded that filaments of both glosso-pharyngeal, and lingual division of the fifth nerve, subserved the function in question.[2]

In illustration, however, of the inutility of vivisection as a source of *direct* proof in these researches, nothing is

[1] Anatomy and Physiology of Man. By Todd and Bowman. Part 2d, p. 316.

[2] Panizzas' experiments are related by Dr Burrows in one of the numbers of the London Medical Gazette, for October 1835; the account of Mr Mayo's experiments occurs in a later number of the same year. Dr Alcock read an account of his own proceedings at the meeting of the British Association for the Advancement of Science, in the year 1836; the paper was reprinted in the Medical Gazette in the following December.

probably more striking than Magendie's experiments on the facial branch of the fifth nerve. "I have cut," says he, "the fifth pair of nerves in an animal, and instantly it lost the sight of the same side."[1] From this and other experiments, he threatened, for a while, to supersede the understood functions of the optic nerve; and, indeed, of those of the other nerves of special sense—conceiving the fifth to supply the main requirements, judging from the mere vivisections.

Mutilations of the living brain, as a means of *discovery* in physiology, would, it is conceived, have been long since given up, had their worth been estimated without prejudice; and, indeed, it is only when support has to be rendered to some favourite theory which rests primarily upon facts obtained by this means, that any physiologist of note attaches much importance to them. It is known that even Sir Charles Bell, in his later publications, spoke very slightingly of the method, notwithstanding the comparative success that attended his own employment of it. And, in the rising school of more modern physiologists, it is gratifying to notice that Dr Carpenter virtually gives it up as a source of discovery. "It frequently happens," says he, in his Human Physiology, "that when such violent operations are practised on the nervous centres, they occasion an amount of general disturbance, which suspends or modifies functions that have no immediate connexion with the organ in question; so that we cannot safely attribute the alteration in them to the loss of it." And again, "There is much difficulty in ascertaining the really elementary functions of the nervous system by experiments upon animals; and it is only when their results are corrected and explained by pathological observation on man, that they have much value to the physiological inquirer."

There will, in the sequel, however, be an attempt to

[1] Magendie's Physiology. Milligan's Translation, p. 48.

show that, after the functions of individual parts of the encephalon and of the nervous system have been truly discovered, the results of mutilations harmonise completely with the knowledge which has been obtained by a method of research more justly fitted for its purpose. In the meantime, it is conceived that it may safely be maintained, as a corollary from all that has preceded, that vivisections should never supply the primary fundamental evidence respecting the functions of any portion of the encephalic mass, or, indeed, of the nervous system at large, however they may, in some instances, *confirm* what they must ever be inadequate to *prove*.

CHAPTER III.

ON COMPARATIVE ANATOMY AS THE PRIMARY AID TO DISCOVERY OF THE CEREBRAL FUNCTIONS.

Wherever there are observed sure and unequivocal signs of conscious existence, the presence of a brain and nervous system is inferred. Self-consciousness, in the order of nature, would seem to be universally manifested by the instrumentality of brain; a communication with the outer world being established, and maintained, by the agency of nerves. On these general propositions physiologists are mainly agreed. Among speculative philosophers, some will be found who contend that the soul of man, the immaterial principle of his nature, will, in some of its attributes, display activity in complete independence of bodily organisation; this, however, is a hypothesis which may safely be disregarded, seeing that it is opposed to all conclusions of experience, and to the opinions of those whose antecedent studies give weight to the judgments which they enounce.

The office of the brain and nervous system, then, is to subserve the superior and special properties of animal life; and it is ascertained as a fact—(which in the absence of positive knowledge might have been inferred)—that a remarkable and obviously adapted modification of the subservient tissues coincides with variations in the animal type. Thus, at the bottom of the scale, insects and the mollusca have so inconsiderable a development of the nervous structure, that some have conceived them to be devoid of its crowning constituent, denying that they

possess a *brain*. This, however, must be a mistake; if they have consciousness, it must operate through some organisation, and whatever be the form or locality of the latter, it must, *because of its function*, constitute the analogue of what all acknowledge to be brain in the higher classes of animals. In ascending the scale, and coming to fishes, we observe a decided advance in the organisation of the brain and nervous system. Whilst, in the invertebrata, the actual brain is hardly distinguishable from the ganglial termination of the nerves of sense, in fishes, with which the vertebrated creatures commence, nervous masses, corresponding to the cerebral hemispheres and the cerebellum in mammals, become apparent; and with these coincide higher instincts, and shades of intelligence in its lower forms. The yet higher power, and the varied states of consciousness which birds exhibit, correspond with increased development of the cerebral structure; the proportion of this to the size of the body, to the ganglia of the nerves, and to the spinal cord, far surpassing that which exists in fishes. In the mammalia, the advance which is made in the organisation of brain, is very remarkable. Its magnitude, absolutely and relatively, greatly exceeds the corresponding structure in the inferior tribes; and the hemispheres begin to assume a convoluted and more complex appearance. Indeed, the law obtains throughout the whole animal series, commencing with the very lowest creatures, and ascending till we arrive at Man; the loftier the psychical attributes, the more highly organised is the associated brain.

Not only is there agreement among physiologists with respect to the above general truths; but a yet more striking law, in relation to this subject, seems also to receive a recognition that is all but universal. This law may thus be stated:—SIZE, OR AMOUNT OF NERVOUS TISSUE, CONSTITUTES A DIRECT ELEMENT OF FUNCTIONAL POWER,—a law which brings the brain and nervous system into harmony with all other created things. In physics, the law is obvious at

once. Size of any given body is always regarded as not the least important property, in an estimate of its power; and, in the vegetable world, productive energy is always qualified by size or amount of structure. In the animal kingdom, independently of the nervous system, the influence of organic size upon energy of function, is every where admitted. Capacious lungs are more likely to be vigorous in the execution of their office, than smaller ones. A large heart will usually propel the circulating fluid with more energy than one that is small; and a like principle holds good with respect to the abdominal viscera; in short, observation has revealed the truth, that the law which regulates the vigour and energy of inorganic matter, of vegetable, and general animal life, prevails also in the particular constitution of the brain and nerves. Thus, as a general rule, the sentient nerves bear a proportion in size to the degree in which the power of sensation is possessed. Desmoulins states that, in man, the posterior roots of the spinal nerves, being for sensation, in supplying the superior extremity, have at once an excess of volume and of surface, at least five times greater, both for each individual fibre, and for the bundle resulting from them, than the anterior roots which are appropriated to motion. And this fact corresponds with the exquisite sense of touch possessed by the human hand. The same author further mentions, that the single nerve of feeling ramified on the tactile extremity of the proboscis of an elephant, exceeds in size the united volume of all the muscular nerves of that organ.

As in animals, and parts of animals, possessed of acute sensation, the subservient nerves predominate over those which are for motion, so, in like manner, where muscular power is predominant, the motor nerves are in excess. It is said that in the horse and the ox, the sum of the muscular roots supplying the limbs exceeds considerably that of the sensitive roots; this arrangement corresponding with their imperfect sense of touch, and great bodily

strength. In birds and reptiles, whose scaly skins limit their sensibility, a like preponderance of motor over sentient nerves exists. The tail in some species of monkeys, the wings in some bats, and the claws of certain birds, are remarkable for acuteness of sensation; and, in such cases, the parts are largely furnished with nerves that correspond.

As with the nerves of common sensation, so it is with those of the special senses. The olfactory nerve, as a general rule, bears a fixed relation to the extent of surface within the nasal cavity, to which it is distributed. The pituitary membrane which lines this cavity, always exhibits some proportion, in surface and extent, to the intensity and energy of the sense of smell. The internal nostrils of the North American, who is remarkable for acuteness in this sense, are of unusual size; and the same is said to be true of all the savage tribes, who possess an extraordinary endowment in this respect. The olfactory nerve in Man, and the quadrumana, is much smaller than in the horse, and the dog, and many other creatures. In some animals, the ganglion with which it is in especial connexion, called *bulbus olfactorius*, is so large as to have been mistaken for the brain itself. In the mole it is very large, while the optic nerve is small; whereas in the eagle, these proportions are reversed, the optic greatly predominating over the olfactory nerve. In all these instances, there obtains a striking coincidence between the size or amount of nervous tissue, and the energy of the associated function. The same law is found to prevail with respect to the other nerves of special sense. Wherever a remarkable manifestation of power is discovered, *there* (regard being had to certain qualifying circumstances), will a predominating nervous apparatus be always found. Thus it has long been known, that most mammals exceed man in the acuteness of their hearing, and they equally excel him in the size of the auditory nerve. This may be verified by examining that structure in the sheep, the cow, or the horse, in which

instances it is of much greater extent than in the human subject.

These illustrations, however, need not be pursued. The great fact that size of constituent parts of the nervous system influences energy of function, is never questioned, excepting to serve some temporary purpose. All the leading physiologists of the present day distinctly recognise it. "No intelligent person," says Dr Carpenter,[1] "can doubt that, as we descend the scale of being, instinct is gradually superseding reason; and that, in the lowest vertebrata, the manifestations of the latter are extremely feeble, nearly all the actions of life being guided by the former. Now, on looking at the encephalon, we perceive a difference in the relative proportions of its principal divisions, so closely corresponding with these, that it is difficult to imagine them unconnected. In proportion as we descend the scale, we find the cerebral hemispheres diminishing in relative size, whilst the ganglia, at the origin of the nerves of special sensation increase to a remarkable degree; and we cannot, therefore, but consider it probable that these ganglia and tracts of grey matter whose size in man is so trifling in comparison to the bulk of his cerebral hemispheres, are subservient to those instinctive actions which are prompted by sensations, but in which volition does not partake." The *particular* views expressed in this quotation, will be discussed before the termination of the present chapter. The immediate purpose is to exhibit the *general* notions propounded regarding the influence of organic size upon functional power.

In a marginal note[2] that occurs in one of Mr Newport's contributions to the Royal Society, referring to an anatomical examination of the spinal cord of a lobster, which exhibited the motor columns and nerves of the usual size and appearance, but the ganglia of the sensitive columns

[1] Human Physiology, p. 210.
[2] Quoted by Solly on the Brain, p. 38.

exceedingly small, this gentleman asks:—" May we not infer from this fact, that the degree of sensation in the nerves belonging to the spinal column very much depends upon the size of the ganglia, and the quantity of grey matter they contain?"

Dr Todd and Mr Bowman, in their work before quoted,[1] observe—" We think that all observation, both in man and in the lower animals, proves that the energy of any nervous centre always bears a direct proportion to its bulk, whether absolute or relative."

From the foregoing circumstances, a means is considered by many to have arisen, by which a demonstration of the offices of individual parts of the encephalon could be made. Because, in the manifold species of animals, a modification in form and size of the brain and nervous system is witnessed, presenting an obvious correspondence with their position in the scale of psychical endowment, physiologists have attempted from such data to pronounce particularly upon the relations subsisting between structure and function; conceiving themselves to be, in some respects, in a condition to decide upon the office of individual parts of the encephalic mass. The possibility of accomplishing this result, in any supposition, will be discussed presently; meanwhile, it is here contended that, by comparative anatomy as a direct means, all attempts of the kind are likely to continue, as they have ever been, useless and void of all sure and positive issue.

If the natural history of the best examples of every class of animals were complete; if their habits, and feelings, and varying shades of intelligence could all be deduced accurately, through the existing opportunities for watching their actions; and, more especially, if the conclusions regarding these would admit of some definite classification, without which, facts could hardly be multiplied by different

[1] Physiology of Man, p. 367.

observers, so as to admit of comparison with one another; if, in one word, there were materials for some reasonable *psychology* of the lower creation, then the first requisite of success would be possessed. In order, however, that individual qualities, as evinced in the habits to which they systematically led, should, in different species, be successfully compared with some particular arrangement of nervous matter, would it not be necessary that the brain and nerves should be constructed upon some model approximatively unique in *form*, so that analogous parts in different tribes of animals should with certainty be recognised? Should not function and structure both be understood, ere any comparisons between them could be availably made? Again, as another preliminary condition for the successful prosecution of cerebral physiology, through comparative anatomy as the direct way, the influence of *size* of structure upon functional power, needs consideration. It does not require to be *verified.* It is already demonstrable, *but the modifying power of other conditions on it* is what is wanted to be known. If we *could* realise all these prerequisites, cerebral physiology might become, to a great extent, particularised and assured, directly, by the method in question.

But the required conditions most certainly do not exist. Respecting the precise actions of many species of animals, much uncertainty prevails; arising, in some instances, from defective means of observation, and in others from the contradictory accounts afforded by writers of equal weight and authority. On the inwardly conscious states which originate habits and tendencies in the lower animals, doubt must always prevail to an extent admitting of no comparison with that which obtains in our own species; we communicate among each other by means of speech, and we have the experience of our own thoughts and feelings, in reasoning concerning the springs of conduct in others; but this is not our case with respect to the inferior creatures; we can judge of them only from their movements and cries,

and reason from analogy regarding their import. And when did mankind ever agree, in reasoning uniformly from analogy? Thus, with animals, the same actions, when considered in relation to the internal impulses whence they originate, receive in many cases different interpretations from different observers; and again, where concord in this respect obtains in great part, a contrariety will not unfrequently arise as to whether the habits come from instinctive inclination, or from some training of the intelligence. Illustrations of these propositions abound in works on zoology and natural history, and their notoriety renders it unnecessary that they should here be exemplified; they go, however, plainly to show that we neither are, nor are likely to be, in a state for classifying the instincts, the feelings, and intellectual powers of the brute creation, in such a manner as to furnish independent materials for comparing these with separate divisions of the encephalon.

Yet, if this precise classification of the instincts, emotions, and powers, throughout the animal kingdom, were practicable, the difficulty would still remain of fixing upon the particular nervous mass appropriated to each sense or faculty, so long as the basis of our investigations consisted *primarily*, in the comparison of different species one with another. By this means, excepting as a secondary or subsidiary source of aid, we never could be sure that in all, or even in most cases, we had defined the exact analogues of any given structure in various species. As the matter stands, this is sometimes accomplished in general physiology with some difficulty, proceeding, as we do in reality, on a different plan to the one now under discussion. If we could imagine to ourselves, that physiologists were commencing their inquiries *de novo*, without any antecedent knowledge gained primarily by studying the human type, the unfitness of comparative anatomy for constituting the first path to any obscure, and totally undetermined, region

of physiology, would require but little argument for its demonstration. Suppose, for example, we had not made out from studying our own species,—from knowing, by consciousness as well as by general experience, the distinctness of the senses and their organic connexions, how unavailable would have been all our dissections of reptiles, fishes, birds, and mammals ; the difference in form of analogous tissues would probably for ever have interfered with a recognition of their functional identity. Cerebral physiology, with some physiologists, is very much in this supposed state; to this matter, however, ample recurrence will be made as we advance.

Let us assume, for the argument's sake, that a complete and satisfactory psychology of the inferior creation were determined, and, at the same time, that no use could be made of knowledge gained by researches already made on man ; how, in such a case, would the comparative anatomist proceed to compare cerebral structure with cerebral structure in different species, so as to verify their analogies, and to appropriate any given faculty thereunto? Mechanical forms and lines of demarcation would never enable him to do this; in such a point of view, no unity of type pervades the animal creation. Some physiologists, looking only to external resemblance, formerly imagined that that portion of the nervous system termed the great sympathetic was identical with the aggregate system, as it exists in certain lower orders of creatures. It is now, however, generally admitted that, where there exist separate sets of functions, organic and animal, there, also, are to be found two corresponding systems of nerves. Indeed, by keeping in mind, in some instances, the distinctness of function, separateness of the organic connexion has been discovered, where previously it had been scarcely suspected. How would the analogy of the supra-œsophageal ganglia, in some of the invertebrata, with the encephalic masses in the vertebrata, have been determined by mere contemplation of the forms? In the

butterfly and in the sepia, one rounded mass gives origin to the various nerves of special sense; whereas, in the fish, the corresponding structure is divided into several distinct parts, each being connected with some special nerve. Who, judging from configuration and external appearance merely, would pronounce the intracranial structures in fishes to constitute the analogue of the structures enclosed within the head of the higher mammalia? Anatomists of the highest repute, notwithstanding the aid derived from other sources of knowledge, are not yet agreed as to the correspondence which obtains among the particular masses, in a comparison of mammal brains with those of fishes, or in some instances even, on comparing different kinds of fishes' brains one with another. Thus, with respect to the first, or anterior pair of knots of nervous substance, found within the heads of fishes, they are considered by Arasky, Serres, Desmoulins, Carus, and Tiedemann, to be analogous to the cerebral hemispheres in man; whilst Collins, Monro, Camper, Ebel, Treviranus, and Cuvier, regard them merely as connexions of the olfactory nerves. Again, the second cerebral mass is believed, by the last named physiologists, to constitute the cerebral hemispheres; the former group consider it to form the analogue of the *corpora quadrigemina*. The subjoined extract still further illustrates the difficulty of identifying corresponding parts of the brain in different species;—the brain of fishes is in question.

"The tubercles situated on the inferior surface of the brain, and immediately beneath the optic lobes, are generally of small size, and seldom contain a cavity; between them are the infundibulum and pituitary gland, generally of very large proportional size. Respecting their analogies and names very much difference of opinion exists. Haller termed them the inferior protuberances of the olfactory nerves; Cuvier considered them as the two optic lobes; Dr Grant calls them the cerebral hemispheres, and supposes they are the representatives of those parts in the

higher animals; Serres considers them appendages to the optic nerves, and analogous to the tuber cinereum; Vicq d'Azyr, Arasky, and Carus, consider them analogous to the corpora mammillaria of higher animals; Tiedemann does not decide upon this point, but judges (from the situation and form of the tubercles) that the latter hypothesis is the more probable one."[1]

Mr Solly, in treating of the structures in the whiting, which he himself conceives to form the analogues of the cerebral hemispheres, observes that Serres has even neglected to include them at all in his description of this fish; they are noticed by Tiedemann, "who describes them," says Mr Solly,[2] "as analogous to the corpora striata rather than to the hemispheres of the brain, and by Desmoulins to the optic thalami. With all deference to the talented authors of these opinions, I must say that I do not imagine either of these analogies to be founded in fact."

In birds, the encephalic configuration varies considerably from that characterising the creatures below them in the scale. The several ganglia, or accumulations of nervous substance, which constitute the encephalon, are not placed, as in fishes, one behind another, but rather one below the other; the hemispheres being of such relative magnitude as to overlap, in great measure, the ganglia which give origin to the nerves of special sense. Yet the brain in birds does not, for the most part, exhibit any thing of that convoluted appearance observable in the brain of mammals. The cerebellum is of such magnitude, and is so located, as not to be difficult of recognition, although it differs from the same organ in man in one important particular. The lateral lobes are altogether deficient, or as nearly so as possible; there is little beyond the middle portion which corresponds, or, at least, is supposed to correspond, with

[1] Cyclopedia of Anatomy and Physiology. Art. Nervous System, (Comparative). By Mr Anderson.

[2] On the Brain, p. 72.

the vermiform process in the human subject. Still, in actual form, the departure of this organ from the type observable in many other creatures, is great enough to render its identification very improbable, if other aids than those supplied by the mere configuration were not afforded.

In the mammalia, the convoluted character of the cerebral hemispheres distinguishes them remarkably; and the secondary structures belonging to the encephalon, show very decided modifications in outward appearance. When different species among mammals are compared, the form and relative position of constituent parts of the brain vary very considerably. Thus, some anatomists have gone so far as to deny the existence of posterior lobes in the *cerebrum*, excepting in man and the quadrumana, resting this opinion on the fact of the cerebellum being uncovered by the brain; amongst these rank names so high as those of Cuvier, Serres, and Tiedemann. But Gall, Spurzheim, and others, have maintained that such a conclusion is thoroughly vicious; that the horizontal posture in animals, in station and progression, so modifies the relation of the vertebral column to the cranium, that the great opening through which the spinal cord communicates with the encephalon is necessarily thrown further back than is the case in man, and that this circumstance explains why the cerebellum, instead of lying under, is situated behind, the posterior lobes. It is observed by Dr Spurzheim, "Were it thus, the existence of the front and middle lobes in all the lower animals might also be disputed with perfect propriety. The bulb of the olfactory nerve is covered by certain cerebral parts in man, monkeys, and the phocæ; but in the generality of mammiferous animals and birds, it lies, altogether, anterior to the cerebral hemispheres; nevertheless, no one concludes that they therefore want the front lobes."[1] M. Serres, in apparent contradiction with himself, yet says,

[1] Anatomy of Brain, p. 175.

"The encephalon of all vertebrated animals is constructed after one uniform type, and with the same elements."[1] And also Cuvier, with like inconsistency,—"The brains of the mammalia have the same parts as that of man."[2]

Sufficient has been brought forward to show that nothing like uniformity, in outward appearance, obtains with the constituent parts of the brain and nervous system in different tribes of creatures; and that, without some definite and precise knowledge of function to start with, we could never make out the physiology where it was previously unknown, by any study of the forms and the relative position of the several nervous masses. And even if, to some extent, configuration and lines of demarcation did, in every species, indicate speciality and analogy of structure, *size, almost independently of other conditions*, would need to constitute a measure of power, ere physiologists could determine the allied function; or how, in striving to attach this to some particular organisation, could any remarkable energy which an animal might display be compared with some presumed nervous apparatus, unless the development were absolutely more considerable than in others, where the correspondent power was but feebly manifested? Instances already adduced have clearly shown that size of an organ is an *element* in functional energy; yet it is but *one*, however important; for the rule respecting size as a measure of power, constantly requires qualification, and more especially when different species are compared. The ox and the horse, whose brains exceed in magnitude that of the dog, seem much below this animal in the vigour of their psychical powers. The spider, or the bee, whose *brain*, or apparatus through which consciousness is manifested, is among the smallest, far transcends, in signs of intelligence, the various tribes of fishes. Indeed, in estimating the influence of size

[1] Anatomie du Cerveau, quoted by Spurzheim.

[2] Anat. Comparée, t. ii.—The asserted deficiency in the posterior lobes of animals will again come under notice in a subsequent chapter.

upon power, it is obvious, on a moment's consideration, that the extent of equality in all the other conditions must first be ascertained; and it is certain, that it is only when the associated circumstances correspond, that size of structure supplies the true measure of functional power. And how widely do all the conditions vary, when there is question of different species; and yet there are physiologists who would determine the functions of parts of the encephalon by comparative anatomy, as a *direct* means.

It must seem conclusive, then, that every prerequisite of success is wanting for the prosecution of cerebral physiology by such processes. We have no adequate knowledge of the mental functions in animals, to enable us to single them out for the purpose of comparing them with special structures; and, even if we had, these latter exhibit no appropriate similarity in type throughout the animal creation, so as to admit of the required comparison, and identification in various species. The method, however, has been tried; and, now let us see with what results.

Nearly all philosophers who have advanced definitive propositions regarding the relations subsisting between the structure and functions of the encephalon, have started with the assumption that the *intelligence* was a faculty unique, as *consciousness* was simple; thence they have made comparisons which have been devoid of all positive result, and deduced inferences which have not been confirmed by extended observation, and which, in any case, would have been almost valueless in practical application. It is notorious that the conscious attributes, in different orders of being, vary in *kind* as well as in degree; some creatures possessing psychical qualities, of which others make no recognised manifestation. Were it, however, not so, it is yet certain that the intelligence is compounded of many faculties or aptitudes, notwithstanding the undoubted unity of consciousness in the normal state; this is a conclusion in which all coincide, who have made the mind a subject

of deep meditation. Under such circumstances, it is no wonder that all attempts to define the organic relations of the aggregate intelligence should have led to no positive result. There would have been a similar failure in the case of *sense*, in respect of which also, there is unity of consciousness, could it have been studied only in its general signification, without descending to the particularities of sight, hearing, touch, taste, and smell. Such a proceeding very much resembles an effort to explain the physiology of digestion, as though it were a simple function. "If," says M. Georget,[1] "it be easy to explain by one general power alone. the manifestation of intellectual phenomena, it is not less so to explain that of the digestive phenomena, by the general faculty of digestion; yet this general faculty is composed in man of the particular faculties of masticating the aliment, of secreting the saliva, of chymification, of secreting the bile and pancreatic fluid, of absorbing the chyle, and of ejecting the alimentary residue; all of which functions are attached to distinct organs, the action of all of which concurs to the same end."

Proceeding in the way indicated above, assuming the brain to form the simple organ of the intelligence, and its absolute size to determine the functional vigour, Aristotle, Erasistratus, Pliny, and Galen, as well as several modern writers, have laid it down that Man owes his mental superiority, organically, to the possession of a cerebral structure, whose magnitude excels that of all other creatures. As to a cursory observer the fact appears to be even so, the notion, upon a superficial examination, looks plausible enough. Yet, although, in far the greater number of instances the rule holds good, it is liable to many exceptions, and cannot constitute, therefore, the natural law. The elephant, and certain cetaceous animals, have larger brains

[1] Physiologie du Systeme Nerveux, p. 127.

than man. Again, the dog and the monkey, have smaller brains than the horse, the ox, or the ass, and yet these latter are inferior to the former in point of intelligence. Indeed, in whatever way this assumed principle may be tested, it will be found to be vicious in any extended application.

Physiologists, perceiving the obvious fallacy involved in the above rule, and yet certain, from observation, that size of the brain has something to do with the intelligence manifested in the animal creation, have next proposed to estimate it, by ascertaining the proportion which the cerebral structure bears, in magnitude, to the rest of the body. And here, as before, the proposition seems to hold good so long as general results only are regarded; and, at the same time, some of the difficulties, created by the former scheme, appear to be got rid of by the present one; in the case of the elephant, for example,—although this quadruped does possess a brain that is *absolutely* larger than that of man, it is yet smaller in relation to the size of the body. But unhappily for this supposed discovery, it was soon ascertained that the sparrow, the red-breast, the wren, the canary, and some species of monkeys, have brains much larger in proportion to their bodies, than man himself. Here, again, the faulty method of comparing different species, as the primary means of discovery, ended in its customary failure.

Sœmmering, followed by some other physiologists, seeing the insufficiency of this rule, attempted another, yet one still resting on the comparison of things essentially dissimilar, and things at best but understood in their generalities. He set forth that the volume of brain, in relation to that of the nerves, would furnish a true measure of the intelligence; and here, again, failure ensued. The proposition was not based upon any careful analysis of instances, so as, by individual facts, to come at the general issue; but the opposite plan was adopted. It was remembered that, vaguely speaking, man has *generally* the advantage over

animals in the predominance of brain over the nerves; descending, however, to particulars, the rule is not universal; in the monkey, the dolphin, and in many birds, the proportion is higher than in man.

Cuvier and some others have conceived that the proportion between the brain and the spinal cord might furnish the surest measure of intelligence in various species; such a method exhibiting the predominance of the mental organ over the nervous apparatus of the senses. Cuvier himself, however, acknowledges exceptions, and cites the dolphin as one.

Another scheme, had recourse to, among others, by Richerand, consisted in estimating the relative magnitude of the head and face; the degree in which the former preponderates over the latter being supposed to supply the right measure of the intelligence. Thus, man is at the top of the scale; and the most stupid and ferocious animals, with enormous jaw-bones and small brains, are at the bottom; and this fact arises, it is maintained, from the circumstance of their whole existence being concentrated in the exercise of the senses of taste and smell. This very plausible idea, however, will not harmonise with all experience; for in this instance, once more, two sets of facts are compared, not in their nature directly comparable; the face has its own functions to perform, and its size, in a general way, can have but an accidental relation to that of the brain, whose functions are of an entirely different order. If we go no farther than our own species, the failure of the present scheme is at once revealed. Leo X., Montaigne, Racine, Mirabeau, and Franklin, had large heads and large faces; but whilst Bossut, Kant, and Voltaire had large brains, their faces were small. There is no evidence that, in the former named personages, the size of face was any cause of deterioration to their intelligence; or that the latter received any intellectual exaltation, reasonably to be attributed to their smallness of visage.

Camper's celebrated facial angle, as a means of gauging the intelligence, took its origin in notions directly and primarily obtained from comparative anatomy; the angle in question is formed, as is well known, by drawing one line from the incisory teeth, in the anterior part of the upper jaw, to the meatus auditorius, and prolonging another from the same part to the most elevated part of the forehead. The more nearly the angle formed by the union of these two lines approaches to a right angle, the higher will be the degree of intelligence; and Lavater, in conformity with such an ideal scale of perfection, has constituted a progressive series, commencing with the frog, and ascending to the Apollo Belvidere. This proposition has received great attention, even from anatomists and physiologists of great reputation; Cuvier furnishes a long list of animals in its support. Now, although this method leads, in many instances, to correct results, it will not serve for universal application; as a *principle* it is faulty, resting upon vague analogy, and quite incapable of enduring any rigorous test. Thus, the configuration and general mass of brain being the same in two individuals, the one with a large projecting jaw, and the other with a small one, the angle will indicate a difference of ten, fifteen, or twenty degrees, where no difference of intelligence really exists; again, according to Blumenbach, three-fourths of the animals known to man have the same facial angle, with every possible difference in the kind and degree of faculties which they possess; further, in many animals, the outer table of the skull is so far removed from the internal, as in the elephant, for example, that Camper's facial angle yields no index whatever to the development of the brain.

It is certain, indeed, that all the positions just reviewed are baseless and untenable, yet they profess, nevertheless, to shed such light upon cerebral physiology as to render it a subject susceptible of practical application; and are all assumed, more or less, from considerations resting,

primarily, on comparative anatomy. It may be noticed, that the foregoing propositions have been advanced by their authors rather in the semblance of *rules of art*, than definitively set down as *axioms in science.* More recently, however, the functions of separate parts of the encephalon have been expounded, by means of comparative anatomy, in forms professedly scientific; how far the *spirit* of science pervades the doctrines now to be examined, the reader may be in a condition to form some judgment on terminating the present chapter.

Dr Carus, who is Physician in Ordinary to the King of Saxony, has for many years enjoyed considerable reputation throughout Europe, both as a human and comparative anatomist; and, about five years ago, he propounded to the world, in a small volume, the "Principles of a new and scientifically based Cranioscopy."[1] In this work, Dr Carus recognises a three-fold division of the contents of the cranial cavity, grounded on the analogy of that which obtains in the brain of a fish. "In fishes," says he, "the middle portion, that of the corpora quadrigemina, which in man is so inconsiderable, is the most important and most extensively developed; while in the higher order of animals, the anterior mass (the hemispheres), and the posterior mass (the cerebellum), are the most conspicuous. In man, the characteristic feature is the enormous development of the hemispheres. Farther, I have shown that these three cerebral masses, which appear almost in the same relations in the early human embryo as in fishes (that is to say, the middle central mass is the largest), are always to be recognised as endowed with a peculiar function. The posterior cerebral mass is the centre of the primitive fibres of the muscular nerves, and of those of sex. In the middle cerebral portion, the primitive fibres of the reparative organs are collected; while in the anterior cere-

[1] Grundzüge einer neuen und wissenschaftlich begründeten Cranioscopie. Von Carl Gustav Carus. Stuttgart, 1841.

bral mass essentially, we find the primitive fibres of the organs of sense, through the medium of which we derive our ideas of sensible objects, and in a higher degree our knowledge. In short, the three cerebral masses stand in relation to the following psychological qualities:—

" 1. The anterior cerebral mass (or the hemispheres) is related to the power of representing ideas, to that of recognising and distinguishing them, and to that of imagination.

" 2. The middle cerebral mass (corpora quadrigemina) is related to the feeling of the condition of our own organic life (common sensation); and to sentiment, or to the feelings which result from the combined action of all our moral faculties.

" 3. The posterior cerebral mass (cerebellum) is related to will, desire, and the instinct of generation.

"As the fundamental elements of mental life are only three —to know, to feel, and to will—so are these three masses the essential portions of the cerebral structure. From these three proceed the three important nerves of sense, those of smell, vision, and hearing, which again correspond to three great regions of the cranial structure, the forehead, the middle head, and the hinder head."

An analysis, in further detail, need not be made of Dr Carus's scientific cranioscopy. It may be sufficient to point out some few of its gross inconsistencies, and its utter variance with all sound physiological demonstration or reasoning. It is to be presumed that when the author in question speaks of that which he calls " scientific," he does not propose to institute a new system of philosophy as well as of cranioscopy, but that he recognises the validity of the Baconian system, as applicable to the investigation of the economy of nature. As then, in the progress of his work, the influence of organic size in the communication of functional energy is admitted and dwelt upon, it may be well to ask him, whether it be a constant and universal

fact that, according to the development of the cerebral hemispheres, the "intelligence" of the individual is exalted; that, to take his own nomenclature in specifying the faculties appertaining to the intelligence, Conception, (Vorstellen), Perception, (Erkennen), and Imagination (Einbildung), are in corresponding strength and activity, according as the *cerebrum*, or brain proper (being healthy), is large or small? A very few instances, taken from mankind indiscriminately, would disprove such a position at once. If we go to the next conclusion arrived at by Dr Carus, that Feeling is associated with the corpora quadrigemina, where is the proof that energy or intensity of this characteristic bears any such relation? Vague analogies, admitting of various interpretations, collected from the animal kingdom, assuredly constitute no "scientific" basis for any such proposition. Again, in regarding the cerebellum as the organ of the Will and Desire, (Wollen, Begehren,) as well as of the sexual instinct, where is the shadow of proof, physical or metaphysical? Have men strong determination and resoluteness of purpose, in the general sense, *always* in proportion to the development of the cerebellum? What evidence is there to show that Desire, the *subjectively-felt requirement* of any individual, apart from what relates to sex, is associated specially with this structure, or is coincident, in any way, with strength of the Will? We shall see, by and by, some of the proofs which our transcendental philosopher adduces. Meanwhile, let us ask, could a more unsatisfactory series of propositions have been brought forward? Could a more meagre, gratuitous, and inconclusive analysis of the psychical principle have been offered?

We shall now proceed to the mode in which the above notions are illustrated and supported. We are told, then, that a large forehead, for the proper estimate of which minute directions are given, indicates high intelligence, and an appeal is made to well known examples for corroboration of this assertion. Let this particular fact, how-

ever, be as it may, it is absolutely untrue that size of the frontal region furnishes any measure of the magnitude of the cerebral hemispheres in the aggregate, which Dr Carus maintains to constitute the organ purely of the intelligence. It would be a wanton consumption of the time and patience of the reader, to give proof of the justice of the assertion that the size of the *cerebrum* stands in no fixed relation to that of the frontal bone! Again, after having been given to understand that Feeling, in what precise sense Dr Carus very imperfectly defines, resides in the corpora quadrigemina, and that these are developed according to the "dominance of the vegetative life, and of the individual feelings, without enlightenment by knowledge, and without force of will," we are directed to ascertain the capacity of the region enclosed by the parietal bones, in order to ascertain the degree of prevalence of the qualities allocated in the "middle cerebral mass," as he calls the corpora quadrigemina. To this, it may be observed, that the middle region of the brain is certainly in excess over the anterior and posterior regions, when the fulness and superficial extent of the bones by which it is covered are proportionately great, but it is the middle lobes of the hemispheres, and not the corpora quadrigemina, that fashion this portion of the outer head. It would be pure supererogation to go into any defence of this assertion. Power of will, and strength of desire, are to be inferred when a large occipital region is discovered, according to Dr Carus. The "posterior cerebral mass" influences certainly the size of the *basilar* portion of the occipital bone; but not, necessarily, the entire occipital region, which is here presented to us, however, as corresponding in development with the cerebellum.

It is difficult to conceive how a man of Dr Carus's undoubted acquirements can have offered to the world such an incongruous series of propositions as those exhibited to us in this scientific cranioscopy; and yet, within the

last two or three years, it has been published in an English dress by Dr Freund, in the pages of the Medical Gazette, with a prefatory recommendation of its *scientific* pretensions, in contrast with all previous attempts of an analogous character.[1] Respecting the whole scheme, it certainly is no injustice to pronounce it baseless, absurd, and inconsistent; a scheme wherein the facts are only partially true, and, when true, standing in no reasonable relation to the conclusions deduced from them. Can this be disproved? If not, there is decisive evidence of the valuelessness of comparative anatomy, even in able hands, when it is appealed to as the primary source of discovery in cerebral physiology. It is surprising, indeed, to what erroneous views a fault in method will lead the most talented and the best informed. The mind, under such circumstances, is with difficulty turned aside from the anticipated result, even on the presentation of the most glaring evidence.

Yet one other doctrine affecting the physiology of the brain, formed mainly by studying the facts supplied by comparative anatomy, remains to be examined; the one suggested by Dr Carpenter.[2] This doctrine exhibits, in *form*, a much more philosophical character than that of Dr Carus, though bearing some resemblance to it; on

[1] Dr Todd and Mr Bowman, in their recent work, refer to the notions reviewed in the text in the following terms:—"Carus has lately propounded a new Cranioscopy, founded upon a tripartite composition of the cranium, which *bids fair to rival the system of Gall.*" What utter ignorance of the actual character of Gall's doctrine, its origin, and progress, is implied in such remarks! No wonder that men do not accept the physiology of the distinguished German, under such circumstances. Carus's "Scientific Cranioscopy" rival Gall's Physiology of the Brain! *Nous verrons!*

[2] Not only here, but in other places in this work, the author will have to controvert the views of his friend, Dr Carpenter, who, for several reasons, is the last physiologist with whom he would willingly differ. Dr Carpenter's talents and attainments, however, naturally render him most influential with the rising generation; and he is, undoubtedly, one of the ablest exponents of physiological science we at present possess. For

detailed examination, however, it betrays the faults and imperfections which all the others exhibit, developed by a method of investigation which is radically vicious; and, in consequence, it will, in *spirit*, be found to deviate little from its predecessors. In order that no accidental misrepresentation may be made regarding the views of this able physiologist, Dr Carpenter's own words, though previously quoted for another purpose, shall again be given. "No intelligent person," says he, "can doubt, that as we descend the scale of being, instinct is gradually superseding reason; and that, in the lowest vertebrata, the manifestations of the latter are extremely feeble, nearly all the actions of life being guided by the former. Now, on looking at the encephalon, we perceive a difference in the relative proportions of its principal divisions, so closely corresponding with these, that it is difficult to imagine them unconnected. In proportion as we descend the scale, we find the cerebral hemispheres diminishing in relative size, whilst the ganglia at the origins of the nerves of special sensation increase to a remarkable degree; and we cannot, therefore, but consider it probable, that these ganglia and tracts of grey matter, whose size is in man so trifling, in comparison to the bulk of his cerebral hemispheres, are subservient to those instinctive actions, which are prompted by sensations, but in which volition does not partake." From what follows, it would appear that Dr Carpenter, very much after the manner of Carus, regards the corpora quadrigemina, as constituting the organic centre of the nerves of special sense, and the encephalic seat of the instincts and emotions, alike in man and animals. "It has been seen that there is a distinct group of fibres in the medulla oblongata, which has its ganglionic centre in the corpora quadrigemina, and cannot be traced into the cerebral hemispheres; it is reasonable, therefore, to suppose that it

these reasons, it becomes the more necessary that his positions should be disputed when deemed to be erroneous.

is functionally as well as structurally distinct; and no function can be attributed to them with such probability, as that of producing those instinctive and emotional movements of the body, which are excited and directed through the sense of sight."[1] Dr Carpenter, following Flourens, maintains that the cerebellum is the harmoniser of combined movements. Thus, then, according to these views, the physiological division of the encephalon, is, first, into *cerebrum*, the seat of intellect; next, *corpora quadrigemina*, for the manifestation of the Instincts and Emotions; and lastly, *cerebellum*, for co-ordinating the muscular actions.

Now in this theory, there is first of all to be noticed, an entire absence of any definitive analysis of the mental powers; and, next, an arrangement of cerebral parts, suggested mainly, it would seem, by the mechanical arrangements of structure. The cerebrum forming, in some physical points of view, a simple organ is made to constitute the material instrument of the *intelligence* as a faculty unique; the *corpora quadrigemina*, especially in many inferior tribes, present every mechanical indication of distinctness, and they are made to lodge the aggregate of the *feelings* (which unguided or uninfluenced by intellect, lead to instinctive and emotional actions); and the *cerebellum*, a separate organ, is made to preside over muscular equilibrium. Before going into any details in objection to the psychology involved in this doctrine, it may be well to consider, in the first instance, how far Dr Carpenter is warranted by facts, in referring to the quadrigeminal bodies as constituting the central termini of all the nerves of special sense; afterwards, the difficulties of the theory in relation to mental philosophy, and to certain generally admitted principles in physiological science—principles at any rate recognised by Dr Carpenter himself—shall receive attention.

It is to be gathered from the whole tenor of Dr Carpenter's observations upon this subject, that he regards the

[1] Hum. Physiol. p. 211.

corpora quadrigemina as standing in the same relation to the olfactory, optic, auditory, and gustatory nerves, as the grey matter of the cord holds to the true spinal system of nerves; these latter, if Mr Grainger's dissections are to be relied upon, always terminating in particular segments of the said grey matter. But what anatomist has ever traced the nerves of special sense, all, so far central as the termination which Dr Carpenter assigns to them? The present writer would not rely upon his own acquaintance with this matter, if it did not correspond with the statements of others whose special vocation is practical anatomy; as it is, he believes himself to be authorised when he maintains that no reason whatever exists, apart from Dr Carpenter's theory, for thinking any of the nerves of special sense, excepting the optic, run into the quadrigeminal bodies. The olfactory nerves arise from the cerebral hemispheres, and (except through these) have no connexion with the ganglionic masses in question; the auditory nerve arises from the medulla oblongata; and, with respect to the gustatory nerve, whether this be the glosso-pharyngeal, or lingual branch of the fifth, or a bundle made up of filaments from both, there is no tracing its fibres beyond the medulla, and certainly not to the corpora quadrigemina. It is granted that inability, on the part of anatomists, to trace a set of fibres beyond a certain point, is no proof that the actual termination is at that point; until, however, some evidence beyond mere assumption can be adduced to the contrary, it must be taken to be so; or at least no one can, meanwhile, justly reason on the opposite supposition. There is, however, another, and more strictly physiological, difficulty in the way of Dr Carpenter's location of the instincts and emotions, if the principle be kept in view which he himself all along concedes, that size forms a leading element of power; it is this,—there is strong reason for concluding the anterior pair of the quadrigeminal bodies to be already engaged by the sense of sight; a fact which leaves only

the posterior pair for the manifestation of every instinct, and of every emotion; although these *testes*, as they are sometimes called, amount only to about a fourth the magnitude of the *nates*, or optic ganglia. A brief summary of the evidence in favour of the optical functions of the anterior pair of the quadrigeminal bodies rests, may not here be out of place.

In fishes, the optic lobes, the presumed analogues of the human *nates*, very generally bear a proportion to the size of the optic nerves; and this relation becomes particularly apparent in those species which possess unusually small organs of vision, as the eel, or have eyes of different dimensions, as the pleuronectes. In those fishes which have two optic nerves of unequal size, the tubercles in question present corresponding differences in dimensions. In the class of reptiles, the ganglia giving origin to the nerves of vision are in some proportion in magnitude to the development of these nerves. The size of the optic lobes in birds, corresponds to the extent and acuteness of the sense of sight; they are, accordingly, immense in birds of prey, and much smaller in birds not equally remarkable for perfection of sight. Human pathology would seem to confirm the prevalent notion regarding the office of the anterior pair of the corpora quadrigemina; in every case of long continued atrophy of the optic nerve, where the wasting had involved the tractus opticus, Gall and Spurzheim found the nates of the side corresponding to the diseased tract lessened in size; and cases of long-continued blindness have been recorded by them, in which there was marked deminution in their size. Vivisection has also supplied its subsidiary proof; Flourens, Magendie, Desmoulins, and Hertwig, all agree that destruction of the quadrigeminal bodies constantly occasions blindness of the opposite eye. One or two instances in the mole species seem to be opposed to the above facts, their supposed analogues to the quadrigeminal tubercles existing in almost an inverse ratio to their appa-

rent visual powers; this difficulty, however, cannot be regarded as decisive, when it is considered how uncertain is the determination, in many cases, of analogous cerebral parts in different species; and, at the same time, it supplies another illustration of the vanity of going to comparative anatomy, primarily, for *proof* in these researches. Let the case, however, be as it may, it is certain that the great *preponderance* of evidence leads to the conclusion which assigns to vision, as its central seat, the anterior pair of the corpora quadrigemina, which are considerably larger in man than the posterior pair.

If the inference be admitted which disposes of the anterior pair as the ganglia of vision, the human feelings, in all their varied extent and intensity, are, on the principle of size measuring power, of much less importance in the mental constitution than one single sense, according to Dr Carpenter's theory traced to its legitimate consequences, seeing that one-fourth only of the quadrigeminal bodies, the *testes*, is disengaged for the reception of all the instincts, passions, and emotions. It may be maintained, however, that the *nates* have functions beyond their office, in relation to the optic nerves, and that their ministration to the visual faculty is probably their least important destination. It may be so; but as yet the *evidence* is all the other way. Destruction of the sense causes atrophy of the structure; and we have no reason to think that, coincident herewith, any or all of the instincts and emotions sustain deterioration. The contrary would rather be inferred; the feelings are often most acute, and the instincts most remarkable, in blind people, the major part of whose corpora quadrigemina, it is reasonable to suppose, from a great many facts, is in a state of atrophy. Moreover, in birds, the optic tubercles (the *nates*), alone are found; the certain analogues of the *testes* do not exist; yet birds have instincts and emotions. It is observed, however, that up to a certain period of embryonic development, the human corpora

quadrigemina are, as in birds, bigeminal; whence it has been inferred that, in these creatures, the bigeminal bodies correspond with the four met with in other classes; an arrest, so to speak, having taken place in their development, in the case of birds. The probability of this explanation is conceded; it accomplishes nothing, however, towards removal of the main difficulty as to the functions.

Even if it were true that no pre-occupation of the corpora quadrigemina by the visual sense had been made out, the proposition that all the instincts and emotions, alike in man and in animals, had in these ganglia their encephalic location, would yet encounter objections still more serious, and of a character utterly insurmountable; always provided that the principle, now all but universally recognised—that magnitude of organic structure furnishes a most important element of functional power—were not abandoned; and it is upon this principle that Dr Carpenter proceeds all along in the proofs and illustrations of his doctrine.

If we go back to that part of this doctrine which assigns to the cerebral hemispheres the *intelligence*, and determine what are the psychical properties comprehended in this term; if we then come to some understanding concerning the qualities stated to be organically associated with the quadrigeminal bodies; it may tend to prove the complete discordance subsisting between the anatomy and the physiology which this theory involves, to show that, correspondent with the admitted principle of development, no relative proportions subsist between the structure and the functions.

"The psychologist," say Dr Todd and Mr Bowman,[1] "must determine what are, and what are not, fundamental faculties of the mind, before the physiologist can venture to assign to each its local habitation." Although the present writer cannot altogether coincide in this proposition, more especially where it assumes the possibility of settling defi-

[1] Op. citat. p. 366.

nitively the analysis of the mental powers and qualities, anterior to any study of the organic apparatus, he yet conceives that, by recognising the impossibility of any complete physiology of the brain which excludes a mental philosophy that is analytical, it is sufficiently true for the more immediate illustration. Yet, Dr Carpenter, in speaking of *intelligence* as the property of all animated beings beyond a certain height in the scale, furnishes no analysis of this property, but discusses it as one simple function, or power, varying in degree rather than in kind, through the several species of creatures, from a low point in the series up to man. At least, such seems to be the meaning fairly deducible from his language. "By intelligence," says he, "we do not mean, however, the reasoning faculties only, but the combination of those powers which are of an educable character, and which become the springs of *voluntary* action in very different proportions in different animals of the same tribe." To exhibit the general consent which obtains with respect to differences in *kind of intelligence* amongst different species, and to show the unphilosophical character of comparing things utterly dissimilar for the purpose of settling supposed questions of *degree*, would involve a repetition of much of the reasoning that has preceded; the present purpose is rather to show that (not to go farther than our own species), the law of structural development in relation to vigour of function absolutely contradicts and overturns the theory in question. If the direct and comparatively sure evidence, furnished by the human organization, go against Dr Carpenter, it is in vain to appeal for support to the anatomy of inferior tribes and their assumed psychical manifestations. If notions are to be taken up, resting only on certain real or fancied analogies existing amongst the various classes in the animal kingdom, there is no reason why there may not be as many systems of physiology as there are different students; for the perception of, and inference from, analogy,

will ever vary amongst individuals, according to their anterior knowledge and natural cast of thought. A series of facts will seem related to one set of ideas in the mind of one individual, and will present a comparative aspect totally different to another, whose stock of information shall be arranged in another manner. It is direct relation alone, not that of mere resemblance among facts, that can establish any proposition bearing upon the economy of nature. Materials gathered from various sources, having certain general correspondences, do admirably well for the purpose of strengthening and elucidating some conclusion; but facts which are intended for proof, should all present the same relation to the inference deduced.

To return, however, to Dr Carpenter's physiology of the brain. The "intelligence," it seems, constitutes that property in beings which imparts to their actions a *volitional* character; and its organic apparatus is the entire *cerebrum.* The "instincts and emotions" constitute "a mental condition, analogous to that which exists in man, when the emotions, passions, or propensities, are so strongly excited, as to act at once on the body without the intervention of the will;"[1] and these are lodged in the *corpora quadrigemina.*

A very ready method of testing, in some degree, the justice and the accuracy of these views suggests itself at once; it is this; first, to ascertain, at least in a general way, what proportion exists in point of size between the quadrigeminal bodies and the cerebral hemispheres in man; then to determine what relative influence the intelligence and the feelings exert over human actions respectively; and, lastly, to see if this corresponds with the differences in structural magnitude. If the physiology in these respects, assumed by Dr Carpenter, be sound; and if it obtain throughout the whole scale of animated being; it surely cannot fail when its application is made to man.

It is immaterial for the present argument to determine,

[1] Hum. Physiol. p. 208.

with mathematical exactness, the relative proportions in man of the brain proper and the quadrigeminal bodies; it is certain, however, that the former is immensely preponderant over the latter; fifty to one is below the real estimate, yet we will assume it to be the actual one. In accordance, then, with the theory in question, the intelligence in our own species should influence and determine human actions fifty times, where the feelings,—the "emotions, passions, or propensities,"—do the same thing but once. But how stands the fact? We will attempt, in the first place, to come at some more definitive acquaintance with the import of the terms employed; inquiring, at the same time, concerning the proportion in which the faculties appertaining to the intelligence stand to the aggregate of the feelings; and, this being done, we shall be in some condition for deciding whether human actions be *induced* more by a *rational volition*, than by the activity and energy of the *emotions*, *passions*, and *propensities*.

Although Dr Carpenter, in his physiology, associates *intelligence* as a simple faculty with the entire *cerebrum*, yet he obviously regards it, in his *reserved* mental philosophy, as compound in its nature. "We do not mean the reasoning powers only," says he, "but the combination of those powers which become the springs of *voluntary* action." In one word, the *intellect*, in all its aptitudes, is clearly indicated. Now, in any analysis that may be made of the mental faculties and qualities for the purpose of aiding the present discussion, mathematical precision must not be looked for; if less will not satisfy the mind, such subjects of inquiry must be abandoned; we must take the best evidence we can get in these matters; and this must always be made up of propositions to be *estimated* by the mind, not to be mechanically weighed or measured; and accordingly as these receive an approximation to general consent amongst inquirers in this department, should their value as evidence be fixed.

It is beyond dispute that the powers of observation, and of becoming acquainted with what are called phenomena, are the primary faculties of the intelligence, and supply the raw material, as it were, for all intellectual exertion. Metaphysicians, generally, speak of the "desire for knowledge." Lord Kames calls it the "appetite for knowledge." "Language," as a primitive faculty of the mind, is very commonly recognised. Dugald Stewart, in his Outlines, refers to it, "as an auxiliary faculty and principle." Knowledge and language being obtained, *reasoning* takes place, by powers tracing the relations of cause and effect, and determining the analogies which subsist among things essentially dissimilar; a division of the reasoning faculties which is generally acknowledged among metaphysicians, including Bacon, Malebranche, Kames, Locke, and others. Thus, then, "intelligence" would appear to be compounded of the faculties for acquiring knowledge, and for language; and of powers for reasoning upon the knowledge gained, distinguished most commonly into two, and not badly indicated by the terms *comparison* and *causality*.

Now, let us proceed to some definitive account of the instincts and emotions, taking for our guidance, in an analysis, the most general consent which obtains amongst mental philosophers. The "sexual instinct," all will admit to be a distinct feeling; Mr Stewart, in his Outlines, admits it to be so. "Love of the young and helpless," as something distinct from ordinary affection, is recognised by Reid and Stewart. "Attachment," Mr Stewart acknowledges as the "desire of society;" Lord Kames, as an "appetite for society;" it is discussed also, as a primitive feeling, by Dr Thomas Brown. The instinct which leads to contention, "combativeness," is admitted by Reid and Stewart, under the name of "sudden resentment;" and also by Dr Thomas Brown, described as "instant anger;" Lord Kames refers to the feeling as "courage." The destructive propensity, Dr Brown recog-

nises as the "principle of malevolence;" Lord Kames, as an "appetite for hunting." "Acquisitiveness," as an independent feeling, is disputed by Mr Stewart and Dr Brown; but admitted by Lord Kames under the name of a "sense of property," and as "an appetite for storing up things of use." The "disposition for concealment," Lord Bacon minutely describes in his essay "On Cunning." "Self-esteem," Dr Reid and Mr Stewart treat of, under the designation of "desire of power;" Dr Thomas Brown calls it "pride," and defines it as "that feeling of vivid pleasure which attends the consciousness of our own excellence;" Lord Kames refers to it as the "sense of dignity," and again under the name of "pride." Dr Reid and Mr Stewart mention the "desire of esteem," referred to by Dr Brown as the "desire of glory;" Lord Kames calls it the "appetite for praise." "Cautiousness" is described by Lord Kames under the name of "fear." Dr Brown ranks "melancholy" among the primitive emotions; as, however, melancholy is rather a morbid than a natural manifestation, it most probably consists in depraved activity of the sense of cautiousness. "Benevolence" is admitted by Dr Thomas Brown, and also by Reid and Stewart. "Veneration" is treated of by Lord Kames as a "sense of Deity;" and the *devotional sense*, as a distinct sentiment, is recognised by hosts of writers on human character, although not adverted to by Stewart, Reid, or Brown, as an original quality. "Hope," as a primitive feeling, is alluded to by Stewart; and, as a principle which "springs eternally in the human breast," is recognised by all the world. "Ideality" corresponds with Lord Kames's "sense of grace and taste," and with Dr Thomas Brown's "original emotion of beauty;" it is the poetic sense. "Wonder" is noticed by Dr Adam Smith as a sentiment; Dr Thomas Brown admits it as an original emotion; and Lord Kames expressly mentions it as a primitive feeling of the mind. "Conscientiousness" corresponds to the "moral sense" of

most metaphysicians and moral philosophers; Cudworth, Hutcheson, Kames, Reid, Stewart, and Brown, all admit it. "Firmness," perseverance, obstinacy, stubbornness, are recognised by many authors and observers, as fundamental traits of character.

Although the foregoing exposition of the varied attributes of the conscious principle, in our own species, may be far from unobjectionable in several points of view, it may be sufficiently detailed and accurate for the present inquiry, the purpose of which is to estimate the relative bearings which the "intelligence" and the "instincts and emotions" have upon human actions, and to see if these in any way correspond with the proportional development of the cerebral hemispheres and the corpora quadrigemina. These relative proportions may receive an imperfect illustration from the subjoined table, based upon the analysis just furnished.

Intelligence, which includes			Instincts and Emotions, which include	
As	Capacity of acquiring knowledge, Attribute of Language, Faculty of Comparison, Power of Causality,	4	As	Instinct of generation, Love of young, Attachment, Combativeness, Propensity to destroy Sense of property, Disposition to conceal, Self-Esteem, Love of approbation, Cautiousness, Benevolence, Veneration, Hope, Poetic Imagination, Wonder, Conscientiousness, Firmness,	17
	located in the Cerebrum as 50.			located in the Corpora Quadrigemina as 1.	

In the above table, we gain a representation of the "intelligence" by the figure 4, and of the "instincts and emotions" by 17; so that *number* of qualities indicating importance, and size determining power, the organic connexions of the latter should, in extent, surpass those of the former by upwards of four to one; yet the theory now under discussion places them—the *seventeen* qualities—in a structure but one-fiftieth the volume of that which constitutes the encephalic seat of the *four* attributes of the intelligence.

Undoubtedly, Dr Carpenter will object to this mode of testing his physiology; but it is not here proposed as one that is valid and altogether just; and yet it is but his own reasoning traced to its legitimate consequences. Whilst he admits, what everybody knows, that the intelligence comprehends various powers, he supplies no detailed account of them; as, then, he furnishes no classification of his own, it is fair to presume that he refers to the intellectual powers as most commonly recognised and arranged by those who have made mental analysis a special study; in like manner, the emotions, passions, and propensities, are stated by Dr Carpenter only in general terms; and this being the case, the sense in which these are most commonly understood by metaphysicians and moral philosophers, must necessarily be taken as the basis of particular definition. Dr Carpenter lays claim to the character of a scientific writer, and is not sparing in his condemnation of other inquirers who infringe or neglect to observe the rules which should govern all scientific investigations. It appears allowable, therefore, to remind him, that if words be employed only in their *general* signification, without resort to *individualities*, it is impossible to test the validity of any scientific proposition; and it has been shown that the arrangement of individual psychical attributes, adopted above, is most conformable to ordinarily received notions on such matters. Dr Todd and Mr Bowman, who, in their method of deal-

ing with subjects like the present, are very much in accord with Dr Carpenter, insist, as before quoted, that the "psychologist must determine what are, and what are not, fundamental faculties of the mind, before the physiologist can venture to assign to each its local habitation." At any rate, it is quite certain that, before any substantial advance can be made in cerebral physiology, the philosophy of mind must coincidently be studied. No physiological conclusion regarding the mental organs, which glaringly contradicts well ascertained psychological facts, can be true.

Dr Carpenter may contend that numerical superiority, on the part of one set of attributes, is no proof whatever of a corresponding superiority, in power or influence, over human actions,—no more than it would be right to say that number of muscles will determine the aggregate strength, seeing that it is energy and intensity, rather than variety of operation, which leads to the estimate of functional power. The validity of all this is conceded at once. To render it, however, applicable to the present argument, it must be shown that, in point of fact, the intellectual suggestions in man do actually exert an influence over his deeds, surpassing that of all his feelings, to the extent, at the lowest estimate, of fifty to one. Surely no one whose habit it is to reflect upon his own consciousness, or to observe human conduct, and, in the process, to trace action to motive, will venture upon an assertion so opposed to the fact. What human doing is not determined, mainly, by some *feeling?* And what is the function, in most cases, of the *intelligence*, but partially to supply the *pabulum* to the inward disposition, and, ultimately, the volitional mandate, in obedience to the predominant emotion? It is very questionable whether the intellect leads *directly* to any action, excepting to such as yield immediate gratification to itself, as particular studies for their own sake. Even in purely intellectual pursuits, the emotions, passions, and propensities, yield

very generally the principal motives. A youth *may* study because he loves the occupation, and for little besides; but he prosecutes science, letters, or philosophy, much oftener, from love of approbation or of gain, or from a sense of moral duty. It is because there is unity of consciousness, and because intellect always supplies the *ultimate* element of *will*, that the relative influence of the feelings over actions is overlooked; just as Dr Carpenter informs us, that the *voluntary* control which is possessed over some of the functions, mainly excito-motory, causes us, in many cases, to be unmindful of their true character, which is essentially *involuntary*.

An objection which applies to locating the intellect in the aggregate of the hemispheres has before been dwelt upon; it may, however, be reiterated in relation to this notion, as propounded by Dr Carpenter. It is this: No constant relation exists between their development and the vigour of intellectual manifestation; an assertion which the daily experience of any observer may confirm; and yet such a relation, *in a single species*, ought to be discovered, if Dr Carpenter's theory were sound. It is quite true, that "as we descend in the scale of being, instinct is gradually superseding reason; and that, in the lowest vertebrata, the manifestations of the latter are extremely feeble;" and it is certain that, "as we descend the scale, we find the cerebral hemispheres diminishing in relative size;" and yet it must now have been rendered plain enough, that such premises do not warrant the inference that has been deduced from them. In a subsequent part of this work, it will be shown that other psychical qualities besides those of intellect diminish as we descend in the scale of being, in accordance with the diminution of the cerebral hemispheres. Meantime, it may be remarked, that Dr Prichard, adopting, for a particular purpose, a similar style of reasoning, observes, in an article on the Temperaments, published in the Cyclopedia of Practical Medicine,

—" When we consider the great amplitude which the cerebellum attains in man, in comparison with its size in the lower animals, we are obliged, if we really attach any importance to such a system of correspondences, to acknowledge some relation between this circumstance and the transcendent superiority of the human intellect, compared with the psychical powers of brutes." And, truly, if *resemblances* among things are to be estimated, in disregard of their *differences*, a warranty might exist for Dr Prichard's conditional inference, that the intellectual powers had some organic association with the cerebellum. But, as it is well remarked by Dr Roget, in his article Cranioscopy, in the Encyclopedia Britannica, " Comparative anatomy, upon which so much is made to hinge, is of all guides the most fallible in questions of this nature;" and this, with all due respect to Dr Carpenter, is as true of *his* cerebral physiology as of that of others belonging to the same school. And when comparative anatomy, as the primary source, fails for its purpose in the hands of one to whose extensive knowledge and superiority of intellect the author can pay a willing and merited tribute, it furnishes to his own mind, at least, an additional evidence that even the highest abilities can accomplish but little in the absence of a just method of investigation.

Dr Carpenter's views regarding the office of the cerebellum, not being peculiar to himself, nor deduced primarily from comparative anatomy, need not in this place be examined; more especially as the physiology which assigns the co-ordination of muscular action to this structure, will receive attention, more appropriately, in another portion of this work.

The physiology of the brain is inseparably associated with mental philosophy. It cannot be otherwise, if the brain be the organ of the mind. And if we would advance our knowledge beyond the general proposition, so as to ascertain the functions of individual cerebral parts, the

study of one species, and that one most appropriately the highest, where anatomical variety and psychical peculiarity can with the greatest facility be compared, should ever precede that of the animal kingdom at large. When researches, for the purposes of direct induction, are made upon different species, facts are gained which can only be compared with the greatest difficulty either in an anatomical or physiological point of view. Their relations one to another cannot always be traced, and so they become utterly unfitted to establish any sure conclusion. Most valuable, however, are they in their legitimate place. When facts from the animal kingdom, relating to the anatomy and physiology of the brain are estimated by the guidance of what is already known in our own species, and when inferences with regard to them rest on their probable analogies to the human type, they are important in themselves, and conducive to the advancement of our general knowledge. Moreover, comparative anatomy constantly strengthens and elucidates truths ascertained, in the first instance, by observations made on the human structure; and where it does not, it may sometimes lead to detection of probable inaccuracies in previous observations on man, bringing about their rectification by the induction of a more strict investigation.

CHAPTER IV.

ON PATHOLOGY AS A MEANS OF DETERMINING THE OFFICES OF PARTICULAR PARTS OF THE BRAIN.

Enough has been advanced, the author conceives, to prove the complete inadequacy of vivisections and comparative anatomy to illumine the overhanging obscurity, when complete ignorance prevails with respect to the functions of any part of the animal organisation, and more particularly in those cases where the nervous structure is involved. It must, indeed, be obvious to all who have reflected much upon this subject, that such methods of investigation can never lead to the knowledge of physiological truths, where previously all was unknown; notwithstanding the results may, and often do, corroborate and elucidate, by harmonising with, the conclusions obtained through researches prosecuted in accordance with a sounder philosophy. How fares it with the third and last rule, advanced by Desmoulins, for investigating the physiology of the brain and nerves? Do the phenomena of disease afford us any better kind of evidence for determining directly the functions of individual parts of the encephalon than that which is gained by mutilations of the living brain, or by the institution of comparisons among the cerebral structures of fishes, birds, and mammals?

The late Sir Everard Home was one of the first to give prominence to pathological facts in these researches. "The various attempts," says he,[1] "which have been made to

[1] Philosophical Transactions for 1814.

procure accurate information respecting the functions that belong to individual portions of the human brain, having been attended with very little success, it has occurred to me that, were anatomical surgeons to collect, in one view, all the appearances they had met with, in case of injury of that organ, and of the effects that such injuries produced upon its functions, a body of evidence might be formed that would materially advance this highly important investigation."

It is a conclusion of experience, as well as a suggestion of right reason, that the natural performance of any function must be associated with, and depend upon, a normal condition of structure; that health, in one word, must arise from integrity of the organisation. It is certain that, when any disorder occurs in the functions—when there is any departure from their proper manifestation—some change in the molecular disposition of the correspondent organ or organs has taken place. If the tegumentary investment of the frame were transparent, so that, with the eyes, we could examine this fact as a matter of direct observation, it would probably be seen that *every* deviation from healthy action involved temporarily some appreciable alteration in the condition of structure. We do not, however, come at this truth by so ready or so facile a process. It is rather an inference that we deduce from the often observed fact, that the molecular change in an organ, going on during life, is at death so considerable as to leave obvious traces when it is examined *post mortem*. Alterations in colour, form, size, and consistence, are frequently noticed precisely in those structures of the body which had evidenced unequivocal aberration of function during life. For these reasons, it has been conceived that, by recording what had gone wrong during life, and comparing this with the necroscopic result, an organ whose office was unknown should have its physiology revealed.

If every alteration in function, as revealed by symp-

toms, were attended with such decided changes in the organisation, that they could be readily ascertained after death; and if, at the same time, the changes in question did not involve organs whose functions, during life, had evinced but little disturbance; there would be some reason for going directly to pathology and to morbid anatomy, in the determination of the unknown functions of any part of the body. Yet, it would be necessary to the success of even this species of investigation, that a knowledge of the functional manifestation should precede the attempt to make out the related structure; or, how could a comparison be instituted? How could we assign function to structure without some such previous knowledge? Two things can never be associated for the purposes of an inference, unless both are, to some extent at least, known and appreciated.

Now, let us, in the first place, inquire whether so constant a relation do really exist between symptoms during life, and appearances after death, as to give any great degree of value to physiological researches prosecuted through this means, in those instances where the primary steps have to be taken. Let us see if such be the case with respect to the organisation at large, and then we shall be in a better condition for applying the inquiry to the particular case of the encephalon.

As a matter of fact, the relations between the main seat of diseases, and the irregular manifestations to which they lead, are most uncertain and variable, bidding defiance to every attempt at classification. A set of symptoms will often display themselves, referable to some obvious change that may be detected after death; and the very same symptoms, so far as an observer can judge, will, in another case, be dependent upon a totally different condition of the structures, as revealed by post-mortem inspection. It will not unfrequently happen that the central disease shall orginate external indications that direct the physician's attention

far more to the organs, secondarily, or sympathetically, affected, than to those which are the subjects of permanent physical change, noticeable after death. Moreover, some very vital structure shall become so seriously affected by disease as to induce a fatal termination, and yet no very material alterations in its appreciable characteristics be afterwards witnessed; and, on the other hand, deep and irreparable changes in the organisation will, at times, have advanced to the most serious lengths, without any very sensible alteration in the functional manifestations. Thus, blindness from paralysis of the optic nerve, dependent upon recognisable fault in its tissue, or in that of its connexions, will sometimes come on; and, at others, the exciting cause may be the presence of intestinal worms, implicating the visual apparatus only by sympathy; and yet the differences in functional irregularity, may hardly be perceptible in the two cases. The author remembers the case of a friend, occurring some years ago, where there had been a long continued liability to paroxysms of intense headache; in one of these, the face became flushed and excited, convulsions ensued, and the patient died. The symptoms had directed the notice of the attendant practitioner to the head; and, when the convulsions occurred, blood was rapidly withdrawn from a vein in the arm, and death very shortly took place, as if from collapse. The writer assisted in the post-mortem examination. Within the head not the slightest trace of disease was to be found; but, on examining the interior of the stomach, its lining membrane was observed to be thickened, indurated, and very slightly ulcerated; and this state of things was considered by all present to have constituted the essential disease. Yet the patient had experienced dyspeptic symptoms only of a very ordinary character, certainly much less severe than such as often coexist with a state of stomach evincing no structural change. Then, again, we have many examples of severe stomachic disorders enduring for years, and even causing death, without

having occasioned any perceptible disorganisation. Cases innumerable of sudden death are on record, wherein no appreciable lesion either of the heart or nervous centres can be discovered. Tetanus and Hydrophobia, which are diseases presenting the greatest uniformity in their symptoms, have not yet revealed any uniform organic changes, notwithstanding the most assiduous necroscopic researches. Such is the uncertainty and variability in pathological investigations, relating to the system at large. How stands the matter when there is question of the brain in general, or of its parts in particular.

If the sympathies between distant and remote organs, in which there is comparatively but little analogy in their respective offices, be so considerable as to have rendered the facts of pathological anatomy the most incongruous of accumulated records; how much more decidedly and strikingly may this incongruity be expected to obtain, when there is question of individual parts of the encephalon, where every condition of sympathy exists to its fullest extent,—where, between the constituent portions of the structure, there is direct communication by blood-vessels and cerebral fibres,—where there is most intimate association among the various functions (an undoubted fact, whatever differences may prevail as to the exact character of these functions),—and where its various parts are packed up in the closest contiguity. The anticipation which any plain understanding would unavoidably form with respect to this matter, receives abundant realization in the best authenticated statements contained in pathological records. As this point of the inquiry is not less important in its bearings upon the subject of this work than the topics discussed in the foregoing chapters, a satisfactory establishment of the position here taken becomes necessary.

The most valuable, the most faithful, and the most impartial accounts, adapted for the present purpose, are probably to be found in the *Clinique Medicale*, of that distinguished

pathologist, M. Andral. A detail of numerous examples, showing the uncertainty that prevails in regard to the relations subsisting between the symptoms of cerebral disease, and the organic lesions discoverable after death, would be an affair of disproportionate prolixity, and incompatible with the general design of the present work; the recapitulatory statements, however, which M. Andral has affixed to various groups of cases, shall be supplied; and in this way, the reader will have exhibited to him all requisite corroboration of what has just been asserted, and have brought before the mind at once their general bearings, which might probably have escaped his notice during a tedious perusal of the cases in detail.

After furnishing particulars of numerous cases of cerebral hæmorrhage, which had induced complete or partial paralysis, M. Andral observes as follows, in recapitulation of the results, so far as these involved the power of voluntary motion :—

"The most characteristic symptom of cerebral hæmorrhage is paralysis. We know of no instance of hæmorrhage, which was not accompanied by a diminution, more or less complete, more or less extensive, and more or less permanent, of the power of motion. In the cases above cited it may have been seen, that a very slight effusion was sufficient to produce paralysis; that in general its intensity was in the direct ratio of the extent of the effusion; that it took place no matter what parts of the cerebral hemispheres were the seat of the lesion; and that, in fine, the differences with respect to the seat of the hæmorrhage had very little influence in determining what parts of the body were attacked with paralysis."[1]

Lesions of sensibility, as dependent upon extravasations of blood within the head, are referred to in the subjoined quotation :—

[1] Clinique Medicale. Spillan's Translation, p. 107.

"These lesions are much more constant in cases of cerebral hæmorrhage, than those of motility; and, up to the present time, it has been impossible to detect, in the nature or in the seat of the alterations of the brain, the cause which sometimes suffers the sensibility to be intact, and sometimes produces its more or less complete abolition."[1]

M. Andral recapitulates the various lesions sustained by the intellectual faculties in the cases detailed, and states,—

"These differences in the state of the intelligence at the time the attack of apoplexy takes place, depend principally on the greater or less extent of the effusion. Not only have we seen the loss of consciousness coincide with hæmorrhage in all possible points of the cerebral hemispheres, but we have even found it in cases where the hæmorrhage had its seat outside the hemispheres, in the cerebellum for example, or in the pons varolii. Dr Fabre has cited the very interesting case of an old man, who died of an attack of apoplexy, accompanied with complete loss of consciousness, in whom the nervous centres presented no other lesion than an effusion of blood into the substance of the left anterior pyramid; a very striking example, no doubt, of the wonderful connexion which holds together, and brings into unity of action, all the parts of the nervous system."[2]

In the lucid summary of a considerable number of cases of cerebral *ramollissement*, the same pathologist, remarking on the influence exerted by this morbid condition, states, that

"The presence or absence of disturbance of the intellect, in cases of cerebral softening, seems to depend much less on the nature of the alterations discovered after death, than on the mode peculiar to each subject, according to which

[1] Clinique Medicale. Spillan's Translation, p. 113.
[2] Op. citat. p. 118.

the irritation extends itself from the soft parts to the rest of the encephalon; the traces of this irritation are not such as the scalpel has been as yet able to discover."[1]

M. Andral observes—what indeed is, more or less, within the experience of most practitioners who, having watched disease during life, have looked, after death, for the visible ravages it may have made—that "the nervous centres may be injured in their functions without the anatomist being able to discover any alteration."[2]

The above remarks and expressions of opinion from one of the leading pathologists of the day, are particularly important from the fact, that they are not mere general observations, but the recapitulation, as before stated, of what had been noticed in cases whose details precede the passages just given; and, from the incongruous character of the phenomena, it is obvious enough that they establish completely the position, taken in the present work, that for the direct purpose of discovery in physiology, they are utterly unavailable. Indeed, so sure has this always appeared, when the subject has been examined dispassionately, that very few theories have been erected upon this as the prime foundation; fewer, certainly, than have been raised upon that of comparative anatomy, or even that of vivisection; although, with respect to this latter, the difficulties in *principle* are precisely similar. Nevertheless, some little has been attempted in this way, which, however, receives its speedy overthrow in appealing to the records of an extended experience. Serres and Foville asserted that lesions of the corpus striatum and anterior parts of the brain are followed by paralysis of the lower extremities of the opposite side, and that lesions of the optic thalamus, and posterior parts of the cerebrum, cause paralysis of the upper extremities,—pathological facts which, if invariable, might justly be regarded as capable of leading to some

[1] Op. citat. p. 167. [2] Ibid. p. 6.

physiological inference. M. Andral comments as follows, upon these phenomena:—

"Some cases have been recently published with the view of proving that paralysis of the upper extremities depends on a lesion confined to the optic thalami, or to the nervous mass situated on a level with and posterior to them, and that paralysis of the lower extremities depends on a lesion of the corpora striata, or of a nervous mass situated on a level with and anterior to them. To determine the accuracy of this opinion, we too have interrogated facts. Now, taking those only in which the lesion was perfectly limited, we found seventy-five of them in which this lesion (hæmorrhage or otherwise) was circumscribed with sufficient exactness to qualify them for solving the question now before us.

"Out of the seventy-five cases, we reckoned forty in which the two extremities of one side were paralysed at the same time. Of these forty, there were twenty-one in which the seat of the lesion was the anterior lobe or corpus striatum, and nineteen in which the lesion was seated in the posterior lobe or optic thalamus.

"Of these seventy-five cases, there were twenty-three in which the paralysis was confined to the upper extremity, in eleven of which the seat of the brain was in the corpus striatum or anterior lobe, in ten the lesion was seated in the optic thalamus or posterior lobe, and in two the seat of the lesion was the middle lobe.

"Again, of these same seventy-five cases we found twelve others in which the paralysis was confined to the upper extremity, in ten of which the seat of the lesion was in the corpus striatum, or anterior lobe, whilst in two the lesion was in the optic thalamus, or posterior lobe.

"From these facts, we are led to the conclusion, that, in the present state of science, we cannot yet assign in the brain a distinct seat to the motions of the upper and lower extremities."[1]

[1] Op. citat. p. 109.

Dr Amariah Brigham, an able physician of the United States, has written a book on the physiology and pathology of the brain and nerves;[1] to the general soundness of the views contained in which book, the present writer bears a willing testimony; he experiences no little surprise, however, that a man of Dr Brigham's information and precision of thought should, apparently, attach considerable importance to pathology as a source of discovery in cerebral physiology. For example, Dr Brigham thus refers to the method now under discussion, as one which "we shall be richly repaid for pursuing. The method I allude to is that of pathological investigation, to careful dissection of the brain of those who have died from affections of this organ, and noticing the symptoms manifested during life."[2] He observes, a little further—"We have already learned something of importance by this method of investigating the subject.

"First, we have, it appears to me, ascertained from pathological observations, that the functions of the cineritious and the medullary portions of the brain are quite different; that the cineritious portion of the brain is more particularly concerned in intellectual operations, while the office of the medullary part is to conduct sensation and volition; that when the medullary part is alone affected, disturbance of motion ensues, but not of the intellect; and that when the cineritious portion is diseased, the intellect is alone affected."[3]

Now, with all proper deference to Dr Brigham, it is most assuredly incorrect to state that any such discovery has been made by any such process. It is a highly probable fact, but hardly one that is actually ascertained, that, in the *cineritious* nervous matter so called, the proper

[1] An Inquiry concerning the Diseases and Functions of the Brain, the Spinal Cord, and the Nerves. By Amariah Brigham, M.D. New York. 1840.

[2] Brigham, p. 48.

[3] Brigham, Op. citat.

functional *change* originates, while the medullary or fibrous substance is for the purpose of conducting the impression or impulse; but this proposition does not yet rank as a recognised truth, and, such as it is, it has not been deduced *primarily* from pathological phenomena. The inference has been drawn from the observation of manifestations occurring in the natural and healthful state, and comparing these with certain peculiarities of the nervous structures with which they have been associated. Thus, in the case of the nerves of special sense, the peripheral expansion on which the appropriate impression is made, is found to be composed of pulpy matter, analogous to the cineritious substance of the encephalon, and the fibres running from this, in the direction of the nervous centres, are seen to lose themselves in the cineritious matter of their corresponding ganglia. In the case of the spinal nerves, there is every reason to believe that the cutaneous filaments expand, on the surface, very much after the manner of the nerves of special sense in their ultimate distribution. It is on the pulpy nervous papillæ that incident impressions are received, whence, by *afferent* fibres, they are conducted to the nervous centres, again the locality of cineritious substance. The muscular nerves originate, likewise, in cineritious matter, from which the motor impulse is very reasonably supposed to arise; this, by *efferent* fibres, is communicated to the subsidiary apparatus, the muscles, where there is considered to be little or none of the pulpy structure characterising the terminal ramification of the sentient nerves, whose office is for the immediate reception of impressions. From these and some other like circumstances, the probable conclusion has been attained, that the respective offices of the two kinds of nervous matter vary as above stated. The notion, however, rests, *primarily*, upon observation and comparison of structure and function in the normal state; and, once obtained, it has received strength and confirmation from numerous

indirect sources, from vivisections, comparative anatomy, and pathalogical phenomena. Since the cerebral structure presents a like arrangement, in respect of its two constituents of fibrous and pulpy substance, the inference has been very reasonably extended to the brain; but it does not go beyond the assumption that the cineritious matter is exclusively for the origination of *functional* change, and the medullary for the *conducting* of impulses or impressions. Most decidedly, there is no warranty, either in pathology or in any thing else, for making the cineritious structure purely for the intelligence, and the medullary for motion and sensation. The fact is simply this: Whatever be the functions of the brain in general, and of its parts in particular, it is highly probable that the true seat thereof is in the vesicular or cineritious substance, and that the office of the fibrous tissue is simply to *convey* impressions received, or impulses originating, in the more essential organism. It would, however, be premature to pursue this theme just now.

The facts which Dr Brigham adduces in support of the position which he has taken, are but circumstances which frequently, but not invariably, happen in partial *confirmation* of the doctrine in question. All the phenomena on which he relies, are substantially comprised in the following quotation taken by him from a work by Dr Stokes of Dublin:[1]—

"The fact of delirium occurring so frequently in inflammation of the membranes of the brain is of considerable importance, as showing, not that membranes of the brain have any thing to do with intelligence, but as supporting the opinions of those who believe the periphery of the brain to be the seat of the intellectual faculties; and here is a fact which, so far as it goes, is in favour of the doctrine of phrenology. If we compare those cases of cerebral disease in which there is delirium, with those in which it

[1] Lectures on Cerebral Diseases.

does not occur, we shall find that it is *most common* in cases where disease attacks the periphery of the brain, as in arachnitis. The cases in which we observe great lesions of the brain without delirium, are *generally* cases of deep-seated inflammations of a local nature, or inflammation of those portions of the brain which the phrenologists consider not to be subservient to the production of mental phenomena. This fact, also, would seem to *confirm* the truth of the opinion of the difference in function between the medullary and cortical parts of the brain. It is supposed that the cortical part of the brain is the organ of intelligence, while the medullary portion performs a different function.

"It is, however, a curious fact, that in delirium the inflammation is *generally* confined to the surface of the brain, and that, in cases of deep-seated inflammation, the most important symptoms are those which are derived from the sympathetic affections of the muscular system."

Such facts, however, as those to which Dr Stokes refers, are utterly inadequate to *prove* any physiological proposition: Neither negatively nor positively, is their influence to be recognised as leading to any *uniform* conclusion. Yet in no other way can *proof* be obtained, and the fact validly substantiated. Things occurring in the majority of instances may be said to be *generally* true; and if they support some view or proposition previously supposed to be established, they furnish *confirmation* of it, but nothing more. Dr Stokes evidently regards this matter very much in the same point of view, by no means considering the pathological phenomena upon which he descants as competent for the purpose to which Dr Brigham would assign them. In the passage just quoted, he refers only to what *generally* happens, and states the result as *confirming* a previous notion. This will be apparent from a careful perusal of this passage; certain expressions occurring in which have been rendered in italics for the purpose of directing attention to this circumstance.

It is, indeed, wonderful how able and thoughtful physiologists should ever have attached the least value to phenomena of this class, as a means of primarily determining the relations subsisting between the structure and functions of the encephalon. When every attribute of the conscious principle—when the whole office of the brain—may be thoroughly suspended by lesions undiscoverable by the scalpel, how shall we be "richly repaid," in this point of view, "by careful dissection of the brain of those who have died from affections of this organ, and noticing the symptoms during life?" When a slight effusion of blood about one of the anterior pyramids has been known to constitute the only change appreciable on post-mortem inspection, though associated with the most complete annihilation of consciousness during life, how can we "procure accurate information respecting the functions that belong to individual portions of the human brain," though "anatomical surgeons" should "collect, in one view, all the appearances they had met with, in case of injury of that organ, and of the effects that such injuries produced upon its functions?" Is Sir Everard Home quite right in declaring that, in this way, "a body of evidence might be formed that would materially advance this highly important investigation?"

Here, again, might be adduced the observations previously made with respect to the impossibility of comparing lesions of particular divisions of the brain with the previous symptoms, so as to come at any knowledge of the exact dependence of function upon structure, without some corresponding attention to the phenomena of mind analytically; a consideration but rarely entertained, either by those who have recommended or practised the method of pathological investigation as a primary means of determining the physiology of the brain.

But, indeed, the method in question is now rarely practised, except for the occasional purpose of defending some

favourite hypothesis, or of doing battle to some doctrine, against which the prejudices are enlisted. When it is regarded abstractedly, or by the light of unbiassed experience, its fallacy is at once perceived and acknowledged.

Mr Solly says, " From repeated experience, I can myself state decidedly, that there are few investigations more unsatisfactory and disappointing in their results than those which have diseases of the nervous system for their subject, in reference to a connexion betwixt disordered function and diseased structure."[1]

Dr Abercrombie, in his valuable work on Diseases of the Brain, expresses himself thus:—" We find the same difficulty in attempting to ascertain the effects of *ramollissement* of particular parts of the brain in producing symptoms in particular organs, as in distinguishing *ramollissement* from other lesions of the brain. Convulsions in the same side with the disease, and paralysis on the opposite side, appear to be frequent symptoms, but are by no means uniform. In several cases the speech is remarkably affected, but they present no uniformity in the seat of the disease."[2]

Lallemand, the celebrated French pathologist, in reference to the study of cerebral disease, says, " It is easy enough, in *theory*, to consider diseases in their simple state—to isolate them in our study, but in practice nothing is more difficult than to meet with a disease exempt from complication."[3]

Bouillaud, who, in practice, would seem on many occasions to have approved of the method now under discussion, adds his testimony, nevertheless, to its insufficiency for the solution of any physiological problem. " It is but," he says, " in accordance with strict truth, to declare that a great many more researches are to be made before we can discover in all cases what are the constant pathognomonical

[1] Solly on the Brain, p. 325. [2] Third Edit. p. 116.
[3] Quoted by Solly, p. 326.

symptoms which correspond exclusively to the lesion of such and such a portion of the cerebral mass."[1]

Dr Otto of Copenhagen, in his Compendium of Pathological Anatomy, observes,—"In no instance do we find greater difficulty than in the brain, in making the results of dissection agree with the phenomena of disease previously exhibited."[2]

To the medical practitioner, Pathology should ever constitute a systematic and unremitting study; but the term comprises vastly more than the facts of morbid anatomy; it comprehends every link in the chain of morbid causation. The first deviation from healthful manifestation, the primary disturbance of function, and the immediate influences to which this is attributable, are, to the practitioner and philosophic physician alike, points of equal importance with the final results of disease, as affecting the organisation in the production of sensible physical changes. If Hippocrates and his school were defective in their pathology by neglecting the latter, it is questionable whether, in modern times, we are not equally open to condemnation for our most inordinate and *disproportionate* attention to it, since it leads, in many cases, to grievous disregard of the former. It has long been the opinion of the present writer, that if the study of pathology were regulated more in consideration of the primary external causes of disease, and *relatively* less with regard to the ultimate internal effects, it would lose nothing of its completeness, and at the same time would yield much more decided utility, by rendering to mankind a *practice* of medicine more just and available, and, in consequence, better calculated to alleviate the ills and the sorrows "which flesh is heir to."

To return, however, from this digression; let us determine the inferences fairly authorised by what has preceded. It

[1] Dictionnaire de Médecine et de Chirurgie Practiques, p. 27, tome 7me.

[2] South's Translation, p. 365.

is, then, certainly no undue or hasty assumption to conclude that pathological researches, like vivisections and comparative anatomy, are but little fitted for the *discovery* of the functions of particular parts of the encephalic mass; that however they may occasionally supply hints that lead to successful results on the application of a sounder method of investigation,—however they may corroborate truths otherwise obtained, they are; in their very nature, unsuitable as *direct* guides in the prosecution of Cerebral Physiology.

CHAPTER V.

ON CEREBRAL DEVELOPMENT COMPARED WITH PSYCHICAL MANIFESTATION IN INDIVIDUALS OF A SINGLE SPECIES, AS A MEANS OF DETECTING THE PARTICULAR PHYSIOLOGY OF THE BRAIN.

If neither vivisection, nor comparative anatomy, nor pathological researches, be adapted to the development of cerebral physiology; if none of these, singly considered, can lead to the determination of structural relation with function, may it not happen that, taken collectively, they are competent to bring about this result? We constantly see evidence adduced, and expressions of opinion ventured, by writers of all shades of opinion, obviously in condemnation of some or all of these methods regarded in the abstract; and yet, dealing with the concrete, the very same writers will very often be seen to recur to them as valid, especially when in conjunction. Dr Carpenter, entering upon the consideration of the functions of the brain, says, "It will be desirable to inquire what may be considered as firmly established, before we proceed with details of a more questionable nature. We shall apply to comparative anatomy, to experiment, and to pathology for our chief data."[1] This would suggest that, however fallacious the results may be, obtained from any one of these sources, nevertheless, if the facts bearing upon some problem were collected from all these departments, a conclusion

[1] Human Physiology, p. 223.

might probably be gained, upon which a firm reliance could be placed.

If, in some one instance, a mutilation of the encephalon led to a definitive issue with any thing like uniformity, if all the known facts of comparative anatomy corresponded, and if pathology constantly supported the inference, it is conceded that then the highest approach to proof would be obtained,—nay, probably actual proof—unless the facts would admit of some simpler explanation, to the exclusion of the supposed inference. But, here, a case is supposed that has never yet occurred; and the actual state of our knowledge certainly does not warrant the expectation that any such case will ever happen. Unquestionably, however, in proportion to the extent that coincidence really obtains among the phenomena furnished by these several departments, is the likelihood of justice in the conclusion; and, as corroboration of inductions gained by a more direct method of inquiry, their value, under such circumstances of coincidence, is proportionately enhanced. Of themselves, however, in their very nature, they do not afford *proof.* It has been established, in the preceding chapters, that vivisections cannot determine the function of any part of the brain, that comparative anatomy is in the same position, and that pathology is infinitely too uncertain in the whole of its data for any such purpose. It might then be contended that three negatives cannot make one affirmative, but such a mathematical style of reasoning is hardly admissible in physiological discussions. But the author can appeal, in support of his own position, to testimony of another kind, to that of past experience; and he conceives that it may safely be asserted and maintained, that no discovery has ever yet been made in the physiology of the nervous system by the mere methods in question, as they have been applied in the case of the encephalon. As, regarding the nerves, so much is allowed on all hands to have been made out, we gain a very important subject

for comparative examination; and, in the sequel, it shall receive some attention in detail; meanwhile, the author hesitates not to assert that, in neurological researches, experiment, comparative anatomy, and pathology, whenever appealed to with a sure result, have always been in subservience to some previous knowledge, obtained in another way,—knowledge of a character that is not, that cannot be, possessed by physiologists investigating the brain only by the aids in question. At the risk of undue repetition, it must again be said, that if *primarily* resorted to, they can, singly or in unison, supply only hints for further researches, and furnish at best but a high probability. In all investigations of the nervous system, it must be remembered, that knowledge of function has preceded the search for its nerve; not so with the researches of a certain class of cerebral physiologists; they have made nothing like any successful attempt to specify the individual faculties of the mind, in reasoning from vivisection, comparative anatomy, and pathology, regarding the offices of particular parts of the brain; they have not compared *function defined* and *structure demonstrated*; no; in mutilations of the living brain, they have contented themselves with seeing what effect is produced in the gross; in dealing with comparative anatomy, they have traced resemblances, overlooking the differences; and, in pathological investigation, they have, in many cases, associated a general disturbance of the mental attributes with some lesion of brain with which the previous symptoms may possibly have had an accidental, rather than an essential relation.

If it be decided that none of the methods hitherto discussed, as primary steps for discovering the particular physiology of the brain, be valid, and fertile in result, may it not be a question, whether or not means exist at all for determining the relations subsisting between the structure and the functions of the encephalon? May not

some very reasonably contend that, if contradiction and uncertainty prevail with respect to modes of inquiry pursued by master spirits at all periods when such subjects have occupied attention, no confidence can be placed in any method, nor importance be attached to any results? Let the decision of these questions be postponed for a while.

Whenever two things are seen to be associated, so that the presence or absence of one indicates the presence or absence of the other, an idea is at once suggested to the mind, that the connexion is one more intimate than that which mere coincidence involves. The discovery of leaves and branches in the air constantly suggests the existence of roots within the earth; the two things having always been witnessed in association, and there being, moreover, a general relation observable between the state of the former and that of the latter; the conclusion becomes inevitable, that one set of organs is subservient in function to the other. So it is, in some respects, with the brain; if an unusual development of part of this structure always coincide, under given circumstances, with some remarkable energy in mental manifestation, the inference is naturally deduced, that the two circumstances are not accidents of coincidence, but that the particular power is in some dependence upon the development.

"*Tenterden steeple is the cause of Goodwin Sands,*" is the sarcastic rejoinder, ever ready, when it is advanced that two sets of phenomena exist to each other in the relation of cause and effect, in case the connexion be not readily apparent. The fixed stars appear to us always coincidently with some of the planetary bodies, and yet we do not on this account recognise any close dependence of the stars on the planets, or the planets on the stars, or, in a general way, any thing beyond mere coincidence in their joint appearance. If, because to human perceptions the stars become luminous after the larger planets, some one were

to advance that the light of the former is in any way attributable to the latter as its cause, an appropriate reply might be formed by employment of the adage concerning the steeple of Tenterden and the Goodwin Sands. The case of cerebral development and mental manifestation, however, is, in some sense, that of the root and the branches, not that of the planets and the stars. If the root sustain detriment, the effect is witnessed in the dependent branches; and, contrariwise, if the former be favourably conditioned, the effect is witnessed beyond itself in the latter; and so is the root demonstrated to be an organic requirement, in life, of the leaves and the branches. The movements of the planets, the disappearance of some and reappearance of others, leave the light of the stars uninfluenced; and the condition of Tenterden steeple maintains no sort of correspondence with that of the Goodwin Sands. Development of the brain, however, constantly influences the manifestations of mind; if the quantity of cerebral structure in particular regions be considerable, some related power or quality of the conscious principle will, under ordinary circumstances, be displayed in unwonted energy; and, with great deficiency in volume of brain in the same region, the corresponding power or quality will in every case be but feebly manifested; and not only so, but changes wrought in one set of conditions will often be recognisably induced in the other. Under all these circumstances, the idea of causation becomes irresistible; the induction, that the particular power is organically connected with the particular structure, is philosophically gained.

It is even so with physiology in general. The existence of some distinct organisation, constantly in connexion with some speciality of office, has induced the recognition of mutual relation. Large size and perfection of structure, associated with great vigour and vivacity in function, modifications in the one corresponding with those in the

other—the observation of these things has led to the existence of physiology as a science. For example, the faculty of locomotion, the invariable co-existence of muscles in parts possessed of the endowment, and the reciprocal influence here exercised, readily suggested that the structures were instruments of the power. Their size being seen, in some degree, to constitute a measure of functional energy, there is obtained a means whereby the locomotive power may be inferred from developement of the muscular system. From this, in a great majority of instances, we could say in what parts of the body the greatest strength resided; and, conversely, from the relative power displayed, we could, generally, tell what limbs were most largely furnished with muscle; accuracy in the judgment being the more frequent, in proportion to the extent of correspondence in the allied circumstances. If the same individual were always in question, and if no undue determination of particular muscular actions had preceded, the size would always reveal the power; if circumstances had called the legs into activity to the prejudice of the arms, an equality of the other conditions failing, size, absolutely, would not be sufficient for the purpose. If different individuals were the subjects of comparison, the constancy of result, in attempting to predicate the absolute amount of strength from muscular magnitude, would be less observable; and still less so, if individuals of different species were taken for the relative estimate, because the dependence of power upon size is always modified by variations in the elemental type, in the constitutional peculiarities affecting quality of structure, and in the antecedent training of the individual. These are truths relating to the muscular system, which nobody disputes, and they are justly regarded as resting upon a *principle*—a principle, moreover, which does not apply to the muscular system alone, but which involves the whole range of animal physiology.

It is by application of this same principle to the brain,

and its parts, that we may succcssfully study the functions. By comparing vigorous manifestation of some faculty of the mind with development of some particular cerebral region, and witnessing a correspondence between the two conditions, we immediately fix upon the one as, in some way, connected with the other; and with the more confidence, the oftener the coincidence is observed. When, besides, a serious deficiency in the development is always seen to be associated with defect in the said faculty, the idea of mere coincidence cannot possibly be entertained, that of organic dependence becomes irresistible. By prosecuting such a method of investigation in the case of man only, in the first instance, whcre differences in elementary type do not obtain, and where, generally speaking, there prevails an equality in most other conditions, we make the first advances towards an acquaintance with the particular physiology of the brain; for surely it will not be denied, that, if such comparisons do, in point of fact, produce uniformity of result, the dcsidcratum in this inquiry becomes realised, the true method of determining the relations subsisting between the structure and functions of the encephalon is attained.

In the various attempts of ancient philosophers to promote cerebral physiology, this particular analytical mode of comparing structural size with manifestation of special qualities, was not pursued; and, as a necessary consequence, no particular or precise knowledge of the brain and its offices was gained. The observations were made on the aggregate organ; its constant association with unmistakeable signs of consciousness; the influence mutually exerted between mind and brain in changcs occurring in their respective conditions; the large head, in many instances, indicating great psychical energy, and the diminutive head, as noted by Galen,[1] never being accompanied

[1] Kidd's Analysis of Galen's works. Prov. Transact., vol. vi. p. 328.

by great talents. These circumstances very early settled the question among physiologists, that the brain is the organ of the mind. Beyond this, however, no advance was made. The method, so far as it went—so far as it produced any valid result—was strictly in accordance with the principle here contended for. Yet the observations being but *general*, a general result was only obtained. Still it was the uniform co-existence of brain with mind, and their observed reciprocal influence, that established even the general proposition. Consciousness, or mind, as separate in its nature from the other functions of life, was recognised, and then its relation to certain conditions in the organisation became noted; but no recognition of individual faculties of the mind, in alliance with particular parts of the brain, took place; and thus it was that the ancients never did, never could, advance beyond the general proposition. Up to a recent period, it has been even so with the moderns. Nay, at the present time, some of our most distinguished physiologists, as it has been shown in foregoing parts of this work, are, with all their researches, scarcely in advance of Aristotle in their knowledge of cerebral physiology. They are at a complete stand; and so must they ever remain, if they fashion not their labours to the requirements of the work.

Not only has an acquaintance with the general fact respecting the mind's dependence, in this life, upon the brain been gained by the true *physiological* method, but all our best and surest knowledge of the nervous functions has been obtained primarily in the same way, as will presently be shown. In the ordinary confusion which is made of direct and corroborative evidence, this circumstance is not always attended to. On careful inquiry, however, it will be found to be the case; just as it was seen that all advances in natural science, anterior to the epoch of Bacon, had really been made, not by syllogism or hypothesis, but by the true method of induction, when once

this method had been demonstrated, and been generally assented to.

Unless physiologists consent to abandon attempts altogether to advance the physiology of the brain, *the pressing requirement of the present day, is a settlement of the mode in which the inquiry should be prosecuted.* Concord and unanimity once prevailing upon this point, progress would certainly ensue. Let the results of vivisection, comparative anatomy, and pathology, rank in their appropriate, secondary place, for the purpose of corroborating and elucidating results obtained by comparisons of cerebral development and mental manifestation, and then the entire aspect of this subject, in the regards of a certain class of physiologists, able as industrious, would surely undergo a change, as striking as satisfactory.

If we refer to the past history of physiology, it will be seen that, with respect to the special senses, distinctness of function and separateness of organic apparatus were not discovered by the method that many practise in their searches for the function of particular parts of the encephalon. Function was previously recognised in its speciality, and in a single species, the human; and, owing to the possession, by the special nerves, of an exterior, subsidiary organisation, positive knowledge was obtained in the very morning of existence. Sight was perceived to be essentially different from hearing, smell, taste, and touch; but the idea of distinctness was *placed beyond dispute*, only by demonstration of its special organic connexion. It was the same with the other senses; the function was recognised in the first instance, and the organ, from the peculiar circumstances of the case, was readily associated therewith. If, however, the organic connexions of the special senses could not have been made out, it is by no means certain that the analysis of *sense* would have taken place in any uniform manner. Why should not the sense of *bitterness* have been supposed by some to have differed specifically from that of *sweetness*, just

as much, at least, as from the perception of *fragrance?* It is difficult to see how indisputable demonstration could have had place, without the demonstrable existence of a special organ. Nay, with all our present knowledge upon such subjects, physiologists are hardly yet agreed concerning the specific character of the sense of taste, in the absence of positive knowledge regarding a special nerve of taste. Indeed, at the period of the writer's anatomical studies, the dominant notion was, that taste was *not* a special sense, but only a modification of common sensation. And so it must ever be; distinctness of function can be finally determined only by exhibition of its dependence upon some speciality of structure; but the manifestations of function must fairly be noted, and the speciality be inferred, ere any successful attempt can be made to fix upon the organ; that is to say, the two things must be appreciated, before they can be compared; for, without a comparison, how can the relations of function and structure be traced? In the case of vision, seeing, as a faculty, was, by a mental process, estimated as a power apart; the eye, and its appendages, were observed; the mutual relation of the two sets of facts was noted; the ministration of the latter to the former was inferred; and thus, sight, as a special sense, was *unquestionably* made out, through determination of its organic connexions. Afterwards, whatever was observed in mutilations, in the animal kingdom, and in disease, was found to correspond; and, not only so, but the knowledge in question, by aids of these collateral branches, became corroborated, elucidated, and extended. It should, however, be always recollected, that, before physiological facts gathered from comparative anatomy, and other such sources, can be rendered at all available, the key to their true interpretation must have been obtained from the human type.

The discovery of the distinction between the sentient and voluntary nerves, is an example of research which furnishes no parallel to the proceeding of our modern

vivisectors, comparative anatomists, and pathologists, when they attempt to ascertain the particular physiology of the brain. For, while these neglect entirely the comparison of special faculties of the mind with individual portions of the brain, physiologists, in reference to the nerves of feeling and voluntary motion, had long regarded the functions as distinct, and had even inferred a distinctness in the subservient organisation. It is well known that Hierophilus and Erasistratus, the first known dissectors of a human body, made a distinction between the nerves of feeling and those of motion; and pathological facts had already confirmed the idea, for paralysis of one function was not always coincident with that of the other. Galen, also, was well aware of the same fact,—insisting, from observation and analysis, that every part which is capable of motion, and at the same time possesses sensibility, must receive two classes of nerves—motor and sensitive. The differences between the two functions continued to be recognised during the long period in which the writings of Galen maintained an autocratic sway; and, on the revival of science, Vesalius, Fallopius, Van Swieten, and others, maintained a knowledge of the special fact, though no serious effort was made for the attainment of a more extended acquaintance with the subject. It was not till the time of Gall, that the principle of distinctness of function always being coincident with distinctness of structure in the nervous system, became fairly and systematically insisted upon as a demonstrable truth. In the first volume of his Anatomy of the Nervous System, published in 1810, some time anterior to the published papers of Bell and Magendie, he contends for this principle; and, in illustration, refers even to the double origin of the fifth pair, as correspondent with its combined functions of sensibility and motion. And Spurzheim, some years afterwards, following out the same idea, remarks, three years anterior to the date of Bell's first *publication* on the subject, as follows:—

"The same nervous fibres do not go to the muscles and to the skin, and each of these parts has a distinct function. The nerves which are necessary for voluntary motion cannot propagate the impressions of the sense of touch, nor the latter the impressions of movement. The muscles receive their impressions from within, but the sense of touch from without. * * * It may be objected, that the nerves of motion and of sensation arise from the same pair, and that, consequently, they must be the same. That inference is erroneous; for the fifth pair of cerebral nerves shows the contrary. In fact, the functions of the nervous fibres of that pair are evidently different, and, accordingly, a difference in their fibres is admitted. The pairs of the spinal nerves are divided into different fasciculi, and in a more distinct manner than the fifth, and, consequently, these divisions may also have different functions."[1]

From the above circumstances, it is clear that knowledge of speciality in function preceded the determination of distinctness in the associated structures; and it is tolerably certain, also, that so far as the principle is in question, the discovery which Bell and others, by vivisections, confirmed and completed, was substantially made anterior to their researches and experiments; and made, too, by the combined aids of anatomy and physiology, by the study and comparison of function and structure. Differences in the sensible properties of the muscles and of the skin had been noticed as a fact, functional differences in the subservient nerves had been regarded as a necessary consequence, and the connexion of these circumstances with the separate roots of the fifth and spinal nerves had been reasonably inferred; vivisection, guided by the previous knowledge, and the results being interpreted by its aid, determined the motor functions of the anterior roots, and the sentient properties of the posterior. The *principle*,

[1] Observations sur la Folie, par G. Spurzheim, pp. 26, 27. Paris. 1815.

however, was all made out before; and it is difficult to see in what possible addition to our knowledge, mutilations of living animals could have issued, if the operators had proceeded in the manner of our cerebral vivisectors, arranging within themselves nothing with regard to the individual faculties of the mind, whose aggregate organ they mutilate, in seeming expectancy of detecting function in seeing "what effect is produced" on the body at large. Yet it must be remembered that the immediate subject of this discussion is the grand department of the vivisectors; from the presumed success herein obtained, have encephalic mutilations maintained any material ground in the estimation of physiologists. Yet, allowing the advocates of such a procedure all the advantages which they claim for the vivisectorial method in this instance, no parallel is supplied between the present case and that of the encephalon. This matter is so concisely and so lucidly put by Mr Combe, that the writer has great pleasure in transcribing the following passage from that gentleman's System of Phrenology.[1]

"Some physiologists have endeavoured to discover the functions of the parts of the brain, by mutilating certain portions of it in animals, and observing the effects produced on their mental manifestations. But four conditions are necessary to the success of this method of investigation:—*First*, The part destroyed must be a distinct organ with a specific function; *secondly*, The part injured must be such that it can be cut without necessarily involving the disorder of the functions of a variety of other parts; *thirdly*, If it be nerves that we cut, *the functions of the organ to which they are distributed must be known;*[2] and, *fourthly*, After the operation, the state of these functions must be completely within reach of observation. These conditions were present

[1] Fifth Edition, p. 70.

[2] This is the essential knowledge, obtained purely by comparison of structure with function, as before insisted upon.

in Sir Charles Bell's experiments in irritating or cutting roots of the nerves of motion and sensation. For, 1*st*, These nerves were distinct organs, each having a specific function; 2*dly*, It was possible to cut a branch of the fifth pair, or a root of a spinal nerve, without involving the functions of the nervous system in general in derangement; 3*dly*, It was known that the muscles manifest voluntary motion and sensation; and hence, when one of these powers was suppressed, it was possible to distinguish its absence; 4*thly*, The muscles on which the cut nerves were ramified, were so much within reach of observation, that they could be forced into action or sensation at the will of the experimenter, and hence he could discover the effect of his operations."

It has been rendered sufficiently plain, in a preceding chapter, how very differently all the above conditions of experiment exist in the encephalon; and yet, even in the case of the cerebro-spinal nerves, it has been seen that the co-existence and reciprocal influence of structure and function really afforded the particular knowledge which developed the principle; and this being obtained at first, by study of the human type, it received corroboration and extension from the secondary sources of physiological inquiry.

The existence of a separate system of nerves—the true spinal, apart from the cerebro-spinal—is not as yet universally admitted amongst physiologists; the writer, however, considers it to rest upon evidence quite as valid as that which is generally allowed to authorise other physiological conclusions. And, with regard to this matter, he conceives that it may readily be shown that the leading facts and reasonings, in support of the doctrine, were gained neither by animal mutilations, nor from comparative anatomy, nor from pathology. Before proceeding to demonstrate this circumstance, a brief statement of the modern views concerning the excito-motory function may not be out of place,

as some who may peruse this book, may not probably have attained to any very clear notions respecting it. It is taught, then, by Dr Marshall Hall, Mr Grainger, Dr Carpenter, and others, that, besides the sensory filaments of nerves which pass upwards along the spinal cord *to* the encephalon, and the motor set which convey the influence of volition downwards *from* the same, there exist excitor, centripetal filaments, bound up in the same sheath, which conduct impressions to the grey matter of the cord; that the latter, as a centre of vital influence, reflects motor impressions by centrifugal fibres, still in the same sheath, back again to the muscles; and that this excito-motory function is independent of consciousness, and that the subservient nerves are both anatomically and physiologically distinct from those of feeling and voluntary motion. The general influence of this division of the nervous system would appear to be ancillary to the preservation and conservation of the frame, in presiding over the functions of ingestion and egestion, and by conducing to the repulsion of injurious agencies.

Now, the class of actions deemed to be more especially under the influence of the excito-motory system of nerves, had long been observed and commented upon, anterior to the researches of Dr Marshall Hall; and the inference had been attained, that they were, in an especial manner, dependent upon the physiological agency of the spinal cord. In the opinion of the present writer, the great merit of Dr Hall consists, mainly, in his having more extensively and harmoniously grouped together the related phenomena than any preceding physiologist, and in having insisted (*apparently* at least, though, in his earlier papers, he guarded himself against distinctly *saying* as much) on the necessity of separate nervous filaments for the performance of what was obviously a special function. The *principle*, however, was made out before; the chief facts had been observed; and Dr Marshall Hall is in relation to the

true spinal system, very much in the same position as Dr Spurzheim to the cerebro-spinal nerves. This is an assertion calculated to startle some parties; but let the facts of the case be judged without bias, and it will be seen that the previous extract from Dr Spurzheim's works warrants the writer in what he has stated; moreover, this latter physiologist insists, in the work before quoted, that anatomists should always seek for the existence of separate nerves, upon good evidence being adduced of speciality in function. It is difficult to make out that Dr Marshall Hall did more than this; the *demonstration*, so far as this can be allowed, arose from the researches of Mr Grainger, Dr Carpenter, and Mr Newport, whose labours upon the anatomy were undoubtedly induced by the reasonings and the statements of Dr Marshall Hall.

It has been said, that the class of actions more especially dependent upon the spinal cord had long been noticed. Dr Carpenter, the reputed author of several very able papers upon this subject in the British and Foreign Medical Review, has satisfactorily shown that physiologists, for years before the researches of Dr Marshall Hall, had observed the involuntary character of nictitation, of deglutition, of respiration (essentially), the closure of the glottis, the action of the sphincters, and the reflex movements induced in some cases by external impressions without consciousness. These things had been noticed by Whytt, Cullen, Hunter, Sir Gilbert Blane, and, above all, by Prochaska; some of these writers distinctly referred the influence to the spinal cord, in recognising the involuntary character of the phenomena in question. The performance of reflex actions, moreover, in the anencephalous fœtus—a fact which was well known—rendered the entire notion respecting the independent, involuntary, unconscious agency of the cord, as much of a demonstration as facts could make it. And if the doctrine which regards the vesicular or grey matter of the brain and nerves, as

exclusively the true functional structure, be allowed, Mr Grainger's is the merit of having established (at least if his dissections be confirmed) the existence of filaments in the sheaths of the spinal nerves adapted to the excito-motory office; he having traced, he informs us, some fibres directly into the grey substance of the cord, and others, avoiding it, upwards towards the encephalon. And thus the whole matter, as a subject of modern investigation, would appear to stand:—The vivisections and reasonings of Dr Marshall Hall did much to corroborate and to render more clear what was previously known, suggesting, moreover, anatomical inquiry; this being undertaken by Mr Grainger, a structure was apparently discovered, fairly comparable with the function; researches in comparative anatomy, prosecuted by Mr Newport and Dr Carpenter, still further strengthened, whilst elucidating, the foregone conclusion; and the pathological facts, recorded by Dr W. Budd and others, have contributed additional testimony in support of the same; altogether, such an amount of direct and collateral evidence has been brought to bear upon the subject, that the doctrine of the true spinal system of nerves, and its correspondent functions, is rapidly assuming the character of established truth in physiological science. It must be apparent, however, from what has preceded, that it was by the combined aids of anatomy and physiology, as displayed in the human subject, that the *essential* knowledge was first obtained, without which all the other investigations would have issued purposeless and unintelligible; the vivisections were not practised merely to see "what effect is produced," and thence to have deduced function, but to test an interpretation previously put upon certain natural phenomena that had been observed; facts in comparative anatomy were re-examined, and, in obtaining for themselves a new elucidation, they still further confirmed, in extending the application of, the antecedent conclusions; and pathological phenomena, hitherto obscure,

became explained, and, in the process, contributed to fill up the measure of subsidiary proof.

It is a curious fact, that, in great part, Galen both recognised the function in question, and inferred the existence of separate nerves in subservience to its manifestations. "He conjectures," says the learned Dr Kidd, as before quoted, "that there are nerves for three distinct purposes, namely, for sensation, for motion, and for the discrimination of what may be salutary, and what injurious to the system. * * * He makes a distinction between the powers of voluntary and involuntary motion."

Thus, then, it is apparent that, in point of fact, our actual acquaintance with the physiology of the nervous system has been obtained by a method clearly analogous to that which, in the case of the brain, has been insisted upon as the true one—by inferring from functional manifestation its speciality, and seeking coincidently for some correspondent anatomy.

Assuredly, the particular physiology of the brain can be ascertained in no other way. Its general office being, on all hands, admitted to be in subservience to the conscious attributes, the evidence upon which this leading truth rests needs not receive any additional discussion. It may, however, be reiterated, that it has been made out, and received confirmation, exactly in the same manner as our knowledge of the nerves, by the invariable recognition of consciousness in coincidence with cerebral structure, and their reciprocal influence—a fact which has been supported and corroborated, and its application extended, by all the phenomena furnished by mutilations, the animal kingdom, and disease.

Now, what reason is there that similar evidence should not develope the special functions of individual parts of the brain? Are there any circumstances interfering with the probability of a valid issue, in this particular case? The author sees none. On the contrary, facilities seem to exist,

in some respects, for the purpose, which do not obtain in some other departments of physiology.

If the comparison of mind and brain, in their constant connexion, have justly led to a recognition of the latter as the organic condition of the former, why should not a similar comparison of special faculties of the mind with particular parts of the cerebral mass lead to a like conclusion? Any one at all acquainted with the physical characteristics of the encephalon, knows very well that it is not a homogeneous pulpy mass, as was once believed, but that it is mainly a fibrous, convoluted structure, as fairly divisible into separate portions as the spinal cord, or the medulla oblongata, whose division into parts, functionally distinct, is an undoubted truth. In like manner, every writer on the psychical attributes of man, has distinguished variety, and admitted plurality in the faculties of the mind. This proceeding on the part of metaphysicians and moral philosophers, has been clearly exhibited in a foregoing chapter. From these circumstances, even if we had no other reasons, there would arise a probability, from analogy, that, as the aggregate brain subserved the totality of mental attributes, separate parts were particularly associated with the individual faculties, just as separateness of function in the nerves, always implies the existence of some speciality in structure subservient thereunto.

If the human species had been cast uniformly in the same mould, if either the psychical properties or the cerebral structure had presented no differences amongst individuals, the method now under discussion could not have been applied in the case of the encephalon; and it is difficult to see in what way progress could ever have been made in the present investigation. Neither mutilations of the living brain, nor study of structures presumed to be correspondent in the inferior tribes of creatures, nor the phenomena of morbid anatomy, could possibly have advanced the inquiry beyond some *not unreasonable* conjecture.

It would be tedious to reiterate the reasons for this impossibility. Let us proceed rather to explain how the existence of varieties in the same species conduces to a branch of knowledge, utterly unattainable in a contrary state of things; a knowledge, in a word, of the particular physiology of the brain.

Mankind reason from human actions concerning the motives—the inward dispositions and external influences—whence they spring; and when an individual is seen greatly to surpass his fellows in the energy and the frequency with which, by his actions, he evinces some powerful internal impulse, such an one is regarded as characterised remarkably in a particular point of view, more especially if the resultant conduct seem to have been but slightly induced by external circumstances. For example, a person will be distinguished by his actions, as one that is cautious, timid, and apprehensive, and this, too, under outward circumstances, furnishing no reasonable grounds for such indications; the characteristic feature shall have been displayed in very childhood, shall have marked the boyhood, and the rising manhood of youth, and at all subsequent times shall have stood in relief, in every estimate of the particular qualities of the individual. Every one has met, in life's actual experience, with some such instances. On the other hand, persons are to be observed exhibiting, under all circumstances, a daring and a fearlessness in diametrical opposition to the case just supposed. In this state of things, we always contrast the timid with the fearless person; and the quality, wherein the difference is manifested, becomes very generally regarded as one that is fundamental. In like manner, judging from actions and personal demeanour, we notice and contrast the proud and the meek man, the kind and the unfeeling one, those who are covetous, and those eminently *unselfish;* and so with respect to every thing which, in the language of daily experience, relates to the *natural disposi-*

tion. Not only in this way have metaphysicians and moral philosophers reasoned from actions to inherent faculties or qualities, and to variety in the case of different individuals, but divines, moreover, taught by observation and scripture alike, have usually recognised the same great truth. The *passions* and *affections*, as internal impulses to be restrained and governed, and the *moral sense* by whose guidance deeds should be done, are constantly dwelt upon in their disquisitions; and, not only so, but differences in their inherent strength, in variety of individuals, are frequently admitted and descanted upon. In a volume of sermons now before the writer, the following truthful passages occur:—

"The great Author of nature has given us several different feelings and natural dispositions, for wise and salutary purposes; he intended that we should form these feelings to virtue, and render them instrumental to our eternal salvation, by directing them to their proper objects; but, alas! in the present state of corrupted nature, they are generally turned into vices, degenerate into disorderly passions, and are made subservient to numberless sins. Amongst the various passions that men are subject to, there is one passion in particular that commands the rest, and sets them all in motion; this passion is called the predominant, or prevailing passion, because it is more strong and more lively, more violent and more imperious than the rest; it is it that usually forms a man's character, disposition, and the complexion of his temper. * * * We are to examine carefully what particular vice influences our actions most, what sinful habit has the greatest empire and ascendancy over us—for the predominant passion, or prevailing vice, is different in different persons, according to the difference of their humours and natural dispositions. In some, the predominant passion is an overbearing pride, and an insolent haughtiness; in others it is an insatiable avarice; in some it is a restless ambition; in others, a

brutal lasciviousness; in some, a criminal excess and intemperance in eating and drinking; in others, an implacable hatred, anger, and desire of revenge, or some other disorderly passion."[1]

It is the same with the intellectual powers as with the feelings; their essential differences in kind, and in individuals their differences in degree, are matters of constant observation. One man is profound in thought, and subtle in reasoning, whose stock of knowledge is not in the same proportion; whilst another shall have acquired, with infinitely less pains, double the information, having no ability to trace this to its consequences. Some person shall be deeply versed in mathematics, whose inaptitude for acquiring other kinds of knowledge, or for reasoning upon general subjects, would be deemed incredible in the absence of repeated experience. And so it is *in extenso;* talents, of every sort, vary both in kind and in degree; a truth so obvious, on referring to all past experience, as to require but little further illustration. "Some persons," says Dr Alison, "are strongly impressed by differences of colour, some of forms; the minds of some dwell habitually on certain abstractions, such as numbers, or mathematical figures; those of others on certain of the properties of external objects; those of others on the words by which thoughts are expressed, &c."[2]

In the general fact, that individuals present differences among themselves in their moral qualities and intellectual powers, all writers are pretty well agreed; and it is very generally conceded that these differences, to some extent, are innate. As to what constitute the particular qualities and faculties to be esteemed fundamental or primitive, concord does not obtain; and it is impossible that it should do so, so long as they are regarded apart from the organisa-

[1] Sermons and Moral Discourses. By the Rev. W. Gahan. Dublin. 1836.

[2] Physiology, p. 225.

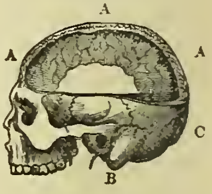
tion; because, however clear and satisfactory to each philosopher his own analysis and definitions may seem, there is nothing, in the very nature of the thing, to command assent by the force of actual demonstration. To reproduce the comparative illustration, previously afforded; if *sense*, in its general signification, could be supposed to have become the subject of analysis without any guidance from the organisation, there is no certainty that sight, hearing, smell, taste, and touch would, as specifically distinct perceptions, have uniformly constituted the group. Assuredly, it has been owing to the allocation of particular senses in separate structures, readily observable, that an unquestioned analysis of sensation has been obtained. And, through a corresponding state of things, in the case of the encephalon, was the immortal Gall led to the discovery of the particular physiology of the brain, and at the same time to a systematic demonstration of the true method of prosecuting such researches.

Gall compared the development in individuals of parts of the brain with some energetic manifestation of power or feeling, and, in deducing the necessary connexion, regarded the cerebral structure as the organic condition of the associated faculty. In some instances, he was first struck with the unvarying predominant energy of a particular capability or disposition, as evinced by conduct, and then he sought to determine the portion of brain correspondingly in excess; and, in others, having noticed the development at the onset, he perseveringly sought for the psychical quality that distinguished the possessor. In the early steps of discovery of an organ, the first idea always arose in one of these ways, on the attainment of which, he appealed to multitudes of facts *indiscriminately obtained;* when large numbers were accumulated, all suggesting the same conclusion, he would regard his provisional inference as probable; and he would only venture to pronounce any proposition to be fairly made out, after the most extensive

and scrutinising observation. It is stated that, at times, after his collected facts had for a long time pointed to some particular notion, the occurrence of one certain, unequivocal exception, always caused him to start anew.

It may be conceived that the task of seizing the striking and salient points of character could very readily be performed; some, however, may not so easily see how these could be compared with the development of particular parts of the brain in the living subject, seeing that the aggregate mass is excluded from sight, by interposition of its bony investments. In the human species, this is but a slight affair, as, by concurrence of all distinguished anatomists, the configuration of the head is allowed to be formed pretty regularly by the development of the brain; this, however, is not the case with mathematical exactness, because the two tables of bone, which enter into the constitution of the skull, are not uniformly parallel. But, except in some instances of old age and disease (excluded by Gall as *evidence* for, or against, his doctrine), the divergence in parallelism is rarely so much as a line, whilst variations in the cerebral development of particular regions will, very often, exceed an inch; so that, for all practical purposes, the form of the head may be deemed coincident with the figurate surface of the brain.

The annexed woodcut will render this apparent in some degree.



This figure exhibits a skull with the two sides cut away, down nearly to the level of the eye-brow, leaving a narrow ridge in the middle of the top standing. A A A is a section of the skull, resembling an arch; it is here represented thicker, in proportion to the size, than it is in nature, in order to exhibit the better the separation of the two tables whose interspace contains a spongy substance called the *diploe;* the average

divergence in parallelism, however, is faithfully represented. The skull is not opened at B C, where the cerebellum lies. It is obvious then, that, whenever there is question concerning the magnitude of the cerebral convolutions in any of its regions, posteriorly, superiorly, or anteriorly, the projections of the cranium in any particular direction will furnish the measure. Indeed, the fact touching the parallelism of the encephalon and the outer head, is but seldom controverted; and, when it is so, it is seen to be for some temporary purpose. It is taught not only by those who admit, but by those who reject, the physiology of Gall. Thus, Cuvier states that "The brain moulds itself in the cavity of the skull, which *it fills exactly* in such a manner that knowledge of the bony part gives us information at least of the *form of the exterior of the brain.*" Magendie says, that "The only way of estimating the volume of the brain in a living person, is to measure the dimensions of the skull; every other means, even that proposed by Camper, is uncertain." Sir Charles Bell, in his *Anatomy*, also observes that, "The bones of the head are moulded to the brain, and the peculiar shapes of the bones of the head are determined by the original peculiarity in the shape of the brain." Dr Gordon, in his day, notoriously the most virulent opponent of Gall's doctrine, states, in an article written by him, which appeared in the forty-ninth number of the Edinburgh Review, as follows:—"But we will acquiesce implicitly for the present in the proposition (familiar to physiologists long before the age of Gall and Spurzheim), that there is, in most instances, a general correspondence between the size of the cranium and the quantity of the cerebrum; that large heads usually contain large brains, and small heads small brains." More recently, in the Cyclopedia of Anatomy and Physiology, it is observed in the article CRANIUM,—"A comparison of the external and internal surfaces of the cranium establishes the fact, that there is a general correspondence of the two,

as far as regards those parts which are in contact with the periphery of the brain." The authorities might be multiplied, but this topic need not be pursued.

From the preceding statements, the method of Gall must be intelligible enough. In *discovery*, he noticed remarkable manifestation of some particular mental characteristic in individuals, in which procedure no great difficulties were in the way; this he compared with some prominent development of brain, to the detection of which it has been shown that the skull presented no obstacle; on the presumed ascertainment (in the normal condition of things) of invariability in the association, the conclusion was drawn, that the one circumstance stood in the same relation to the other, as that in which a particular sense stands to its correspondent nerve. He then appealed for confirmation and extension of his induction to all collateral sources of evidence. The inferences in this way deduced are, in all respects, as just and as valid, as those which have been obtained, and are generally admitted in other departments of physiology. Disease, old age, or some congenital malformation, vitiating the intimate structure of the encephalon, or inducing abnormal deposit of bony matter, so as to destroy the parallelism of cranial form and cerebral development, will interfere with the uniformity of result in the attempt at positive illustration; and, in a very few cases—certainly not one in fifty—a great development, truly cerebral, and dependent upon no undue presence of bone or other obvious indications of disease, will not correspond with its ordinarily associated condition;—just as, in some cases, large muscles will be unequal to the power most commonly witnessed in structures of like magnitude; or in the same way as the optic nerve will occasionally be amaurotic, partially or entirely, though no very obvious lesion be discovered after death, nor consequent derangement of the health have been witnessed during life. These *disturbing* influences, however, can only affect *positive*

results; *negatively*, there is invariability in the sequence; when the cerebral organ of some given faculty is ascertained to be much below the average size, its manifestations, in a high degree, are *never* to be witnessed. In this point of view, unqualified demonstration may be had; the fact is universal. The reasons of the differences affecting positive and negative cases are clear enough; a large amount of brain *may* be unequal to its customary power from defective quality, —a small one can *never* display remarkable energy; as with the muscular system, a large development of some particular muscle does not *always* coincide with the expected strength, but a very small muscle will *always* be relatively weak.

However plainly to most minds the premises, if accurate, suggest the conclusions arrived at by Gall, there are some physiologists apparently disposed to regard the matter more as an affair of coincidence than of consequence, as more in parallelism with the Goodwin Sands and Tenterden steeple, than with the general physiology of the nervous system. Dr Carpenter, in his Human Physiology, observes,—

"It may be freely admitted that mankind is in the habit of forming an impression of an individual's intellectual capacity by the height and expansion of his forehead; and that a low forehead and crown, with great development of the occipital portion of the brain, generally accompanies a character in which the influence of the animal passions is predominant; and correspondences even more detailed may be admitted, without the inference being then conclusive, that these several parts are the distinct organs of the several faculties, or that the size of the organ is a measure of its functional power. It may be thought to be, in regard to the form of the head, very much as in respect to the character of the face,—that we may draw from it a general idea as to the character of the mind, and may not unfrequently be able to predicate correctly some minute details; and yet that an attempt to localize the organs more minutely

may be as destitute of truth as were the details of the system of Lavater."[1]

It should be remembered, however, that the occurrence, in nature, of coincidences with undue frequency, must ever involve some *law*. Mere accident can never explain the almost invariable association of two sets of conditions in the routine of natural phenomena; and if a well developed forehead be *habitually* regarded as indicative of intellectual capacity, and if great development of the occipital region of the brain, with moderate size of the anterior, superior portions, *do* "*generally* accompany a character in which the influence of the animal passions is predominant," some principle must hold these facts in subservience; and what is the principle more obvious and rational than the fundamental axiom in the special physiology of Gall, that size of cerebral structure, *cæteris paribus*, constitutes a measure of functional power? an axiom which, so far as size is in question, is very generally recognised among modern physiologists in relation to the nervous system at large, and, amongst others, by Dr Carpenter himself;—to an extent, moreover, which the disciples of Gall would not admit, as they always demand an equality in the associated circumstances, regarding mere size but as *one*, though an important element. Lavater's physiognomy has no parallel in Gall's physiology. There is absolutely no evidence at all to prove regular correspondences between the features of the face (excluding the forehead) and mental characteristics, apart from *expression;* no form of any individual feature maintains, positively or negatively, any thing like an invariable connexion with the intellect or the disposition, enabling us to "draw from it a general idea as to the character of the mind." Will Dr Carpenter affirm that there is a particular form of nose, or cheek, or chin, which as generally accompanies a particular disposition or in-

[1] P. 237.

tellectual talent, as a large forehead does superior intellectual power, and *vice versa?* Such a state of things obtains only when the encephalon is in question, proving, so far as evidence can demonstrate any physiological proposition, not only that the brain is the organ of the mind, but that it forms a congeries of organs, the function of each being to manifest some particular power or disposition.

Certain minds that have been habitually intent upon the more exact sciences, such as mathematics or astronomy, may not unreasonably be expected to find that sort of proof insufficient, of which Gall's physiology is susceptible. Its just and actual force can be appreciated by those only whose pursuits familiarise them with the phenomena of life, the study of which so rarely affords that full and complete satisfaction to the rigorous mind, which some other sciences afford. The imperfection in question, however, is not peculiar to the doctrine of Gall; it is common to almost every department of physiology. As a striking illustration of the want of acquaintance with the actual character of all physiological evidence, the following extract is taken from a critical notice of the "Vestiges of the Natural History of Creation," which appeared in the North British Review, for August 1845, generally ascribed to Sir David Brewster:—"Every branch of study that deserves the name of science, has its system of facts and its code of laws; but phrenology has never yet been able to adduce a single indisputable fact in favour of its doctrine." What is the invariable and unmistakeable absence of great intellectual power, when the forehead is low, narrow, and receding? It is an "indisputable" fact, and one that is obvious to every *observer*. The reviewer proceeds—"Its object is to discover a relation between certain intellectual and moral truths, and certain physical magnitudes; and yet it does not directly compare those truths with those magnitudes, but with certain other magnitudes, supposed to be similar, and with which the truths in question have

no connexion." Here is a grievous confusion of thought. The reviewer, it is to be inferred, alludes to comparisons of psychical qualities with cranial configuration; but he ought to have known that phrenologists attach no importance to the latter, but in so far as it supplies the index to the cerebral development, which alone is considered in the induction, and with which the mental properties, "the truths in question—*have* connexion." He continues—"In making such a comparison, we must be sure of the correctness of what we assume to be truths. The mental or moral phase must be so prominent and unambiguous, that every man can recognise its existence; and the corresponding magnitude must be so distinctly marked, that all men can see it. The feature in the brain must not be inferred from the feature in the external cranium, covered, as it may be, with hair, and flesh, and skin, and possibly exaggerated or diminished by some external cause. It must be directly observed in the healthy brain itself; and if the cerebral development corresponds in magnitude with the mental feature, we become possessed of a single fact, half moral and half physical. If this correspondence is invariable in all other cases, we have then one phrenological fact in reference to one portion of the brain, which, if, as we assume, it rests on accurate observation, we can compel every sound mind to believe. But if there be one distinct and unambiguous exception, for which no reasonable cause can be assigned, the whole doctrine must be at once abandoned. Were there a single satellite in the solar system, whose motion did not correspond with the universal law of attraction, even the doctrine of gravity must be rejected. How difficult, then, must it be to determine phrenological facts, and in what storehouse can we expect to find them? Who could venture to record it as a scientific truth, that Voltaire had not, and that Sir Walter Scott had, piety as an element of his mind; and that the brain of the one wanted, while that of the other possessed, the

corresponding elevation? Who can testify to us as a fact, that a murderer possesses a truly murderous disposition, or a thief a truly thievish one? It is only insulated acts, and these generally not the result of habit, but of momentary impulse, of which man ever takes cognizance. It is God alone that can pronounce upon the real condition of the heart and soul, out of which are the issues of life. A true phrenological fact, therefore, which we can force a sound mind to believe, must involve in one of its aspects a species of knowledge which it is not in the power of man, and still less within his province, to attain; and in the other, a physical fact, which can be seen only in the brain itself, and which cannot be inferred from any external sign. For such facts, anxious as we have been to find them, we have long sought in vain."

The applicability of principles of inquiry thus rigorous and exact, to sciences like mathematics or optics, must be allowed to be clear enough; if, however, such canons of evidence were enforced in researches into vital phenomena, then adieu to all physiology, natural history, and medicine; scarcely any *science* with respect to any of these could possibly exist.

By whomsoever phrenology, or Gall's doctrine concerning the brain, shall receive ample and complete attention in a serious and philosophic spirit, with perfect immunity from the prejudices that result from anterior notions or previous habits of thought, to such an one the whole question will present itself purely as a question to be determined by the state of facts; if the instances recorded have been accurately observed, if the premises in their general character be sure, the conclusions follow irresistibly. In a few words, unless rules of investigation apply to the brain's physiology which differ from those relating to the remaining organization, facts will be found to necessitate the admission,—*first*, That the brain is the organ of the mind; *secondly*, That different parts of the cerebral structure subserve the manifestation

of different mental faculties; and, *thirdly*, That size of organic apparatus, *cæteris paribus*, constitutes a measure of functional power.

It is idle and vain to discuss any of these axioms without previous acquaintance with the observations upon which they are raised; no appeal to other axioms, however constantly admitted or validly established, can rightly deal with the subject. The facts must be fairly met, and, when these can be disproved, the inferences may be rejected, but not one moment before. The greatest and the wisest cannot meet the question honestly in any other way, or propose one single rational objection in a different manner. The observations should be repeated, whether the purpose be to *learn* or to *controvert;* but before these can be well and accurately made, some preliminary capability, as in every other department of knowledge, must be procured. With respect to this matter, the day is now past for all preposterous nonsense about *feeling if there be any bumps!* Phrenology, or the physiology of the brain, must be regarded "as it exists in the minds of those who have actually studied it, and not in the crude and contradictory form in which it is presented to us by those who have never examined its pretensions."[1] If a *scientific* examination of the facts in question be proposed, in contradistinction to one that is merely physiognomical and empirical, a general acquaintance should be had with the philosophy of mind, so that the inquirer shall be able to recognise primitive faculties when he observes their manifestations, and to distinguish these from mere modes of action or feeling common to the faculties in general; and with the anatomy of the brain, the disposition of its convoluted, vesicular structure, and the relation of this to the fibrous portion, and the ganglionic masses and tracts of grey matter at the base. By this means alone can the true principle, guid-

[1] Dr Combe. On Mental Derangement. Preface, p. xxiv.

ing the estimate of development, be fully and rightly understood. Hereunto should be added a pretty accurate knowledge of the bones of the head, whereby the chance of confounding mere osseous prominence with that induced by development of brain will be avoided. Moreover, as coincidence of cranial form and cerebral configuration is not in every region quite to the same extent, due importance cannot be attached to this circumstance in ignorance of the proper osteology of the head. Further, a familiarity must be obtained with such disturbing influences as create difficulties in the *application*, not only of cerebral, but of general physiology; the influence of constitutional *quality* of brain in modifying the ordinary effects of *quantity*—the influence of disease, that of old age, and of early childhood,—have all to be estimated, before the facts can be rightly dealt with. These things, however, constitute no speciality in the case of the brain; it is so with the muscular system, it is so with the nerves. It must yet always be borne in mind that, both with the brain and with the other structures, the influences in question do but disturb the results in exceptional instances; they must be known and appreciated, however, otherwise Gall's doctrine can never be examined and tested *scientifically*.

Having now exhibited what the author believes to be the true method of determining the relations subsisting between the structure and the functions of the encephalon —a method which *de facto* has ever been applied in physiological investigations that have eventuated successfully,—he shall now proceed to show the results which have been gained by its application, and more especially in the hands of Dr Gall, its *systematic* discoverer.

CHAPTER VI.

ON THE FUNCTIONS OF THE BRAIN, AS REVEALED BY THE APPLICATION OF GALL'S METHOD.

When any doctrine, as yet doubted, is proposed for the examination and consideration of mankind, it is always desirable that those whose attention to it is solicited, should receive some ready guarantee that the probability of truth in the subject in question is such as to render it worthy of inquiry,—that the grounds, at least, on which the proposed doctrine rests, are substantial enough to satisfy the inquirer that he will not idly be wasting time in attempting their verification. In most matters of this kind, certain decided and obvious facts are generally at hand, of a character so easily appreciated, that no obstacle whatever exists to becoming acquainted with them readily, nor any obscurity concerning their veritable import. Under these circumstances, it is always well to appeal, in the beginning, to this class of facts,—to procure a recognition of them, to cause their importance to be justly estimated, and so to furnish *motives* to the inquirer for the further prosecution of the subject; motives which shall constitute assurances that the object sought shall compensate, when found, for the time and the labour consumed in the search.

Now, with regard to the functions of the brain, as revealed by the application of Gall's method of investigation, there exists a facility for supplying the motives and the assurances in question; certain prominent instances may at any time be observed, which are calculated to satisfy every reflecting mind that the principles, at least, of

Phrenology have an actual foundation in nature. Illustrations of this proposition shall be afforded.

The brain, considered as the organ of the mind, may, for this purpose, be divided into three regions; the *first* comprises the anterior lobe, and is subservient to the intellectual faculties; the *second*, the coronal region, is more immediately in connexion with the moral qualities and disposition; and the *third* comprehends the posterior lobe and the base, in subservience to the propensities,—speaking in a general way,—common to man and the lower animals. Now, in a person of well constituted mind, the three regions and the corresponding psychical features are in such relative proportion, as to furnish neither a development that is remarkable, nor mental characteristics of an unusual order, so far, at least, as natural constitution is in question. But let us suppose an individual to exist, who is unmistakeably the slave of low and grovelling passions, in whose bosom no spark of exalted feeling can be recognised or enkindled, and whose intellectual manifestations display but the most wretched imbecility, and we obtain a prominent instance; in such a person, the posterior and basilar regions of the head must be strikingly in excess, or in Gall's physiology there is no validity. Cases like this one, hypothetically adduced, are not difficult to be met with; any large prison, unhappily, supplies too many such illustrations; authenticated casts of notorious criminals confirm the same truth. On the other hand, let some one be selected, whose excellence of moral and religious disposition, and whose intelligence, constitute the leading features of character, and whose animal passions display but little power; in such a case, the anterior and coronal regions will at once be seen to exhibit a marked preponderance. Such facts, to novices in these observations, become the more obvious when contrasted;—extremes on each side.

Suppose that some one, whose thoughts had never been particularly directed to inquiries of this kind, should have

presented to him some such heads as those represented by the annexed woodcuts; suppose that he were asked to declare, judging from the cerebral development, which of the two heads was that of an idiot, and which that of a powerful creative genius; could there be any hesitation in the decision? Most assuredly not. The smallness of brain

SALFORD IDIOT.

MICHAEL ANGELO.

in the one case, and the large size in the other, would be

sure to receive their just interpretation; for all past experience, however little reflected upon, would have exhibited the influence of size upon power; and the amply developed forehead and crown of the one, compared with the small head and diminutive brow of the other, would immediately suggest the importance of cerebral configuration. The genius of Michael Angelo is before the world; the utter idiotcy in the case contrasted is within the personal experience of the writer, who, some years ago, formed one of a society at whose instance the head was moulded, and from the cast the above figure has been taken. If, throughout all time, and in every country, the form of head which accompanies exalted genius contrasts in some such way with the configuration represented above, as being associated with mental imbecility, there is most assuredly a fact incapable of explanation by any notions of mere coincidence; for what in nature is universally true, must always induce the recognition of some principle or *law*.[1]

In this state of things, a circumstance becomes revealed well calculated for supplying a motive to pursue the inquiry; it is not difficult to procure illustration of this kind. If it be of too limited a character to weigh much with the mind, other illustrations can easily be procured, if not quite so striking, yet equally decided. Let any individual enumerate some half-dozen of his most sensible, intelligent, and well-informed friends; let him then select, in like manner, the same number of his most ignorant and

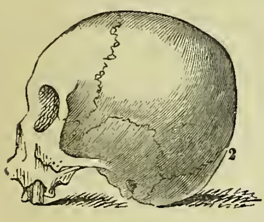
[1] It is not meant that every head, resembling in form and size that of Michael Angelo, will necessarily display the same power; there may be difference in the associated conditions. Every example, however, of such a class of genius will exhibit some such cerebral development. Again, as regards the idiot, every case of idiotcy will not exhibit the style of head belonging to the same category; mental imbecility may be occasioned by faulty condition of structure, as well as by imperfection in development. It is meant, however, that two heads so contrasting in form and size, will always be associated with psychical qualities furnishing, in principle, a similar contrast.

weak-minded acquaintances; and let the development of the forehead in the two groups be compared respectively. Under a moderate equality of all the other circumstances, such as health, age, and general education, a large anterior lobe of brain, comparatively, will *always* be found in the superior class, and a small one in the other. By *forehead*, is not meant the mere breadth and height as regarded in popular physiognomy, because in such a way no accurate measure of the true forehead is gained. It is well known to anatomists, that the anterior lobe of brain rests upon the supraorbital plates, at the posterior margin of which the fissure of Sylvius, so called, divides it from the middle lobe; thus an anterior lobe, whose depth (estimated by its projection in advance of the fissure of Sylvius) is shallow, however expanded so as to give height and breadth to the brow, must, in aggregate size, be really small. It happens very often that such cases are cited in disproof decided, as it is thought, of Gall's doctrine; but the fallacy is perceived at once, when such instances are examined in profile. The distance forward from the lower extremity of the coronal suture is a good indication of the length of the anterior lobe, and will be found to vary considerably, even where the mere fronts look equally large. An excellent illustration of this point is afforded by a writer in the British and Foreign Medical Review,[1] who observes:— "This will be easily understood, by supposing an observer to be placed directly opposite the ends of two logs of wood, each a foot square, but the one twenty feet long, and the other only ten. It is clear, that were he to judge merely from the end view, he would declare both logs to be equal, although, in reality, the one was double the size of the other. It is the same with the anterior lobe; in order to avoid mistakes, its depth or length must be reckoned, as well as its height and breadth. We have heard this called

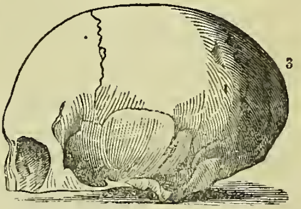
[1] Vol. ix. p. 212. The author thinks it right to state, that the article from which he here approvingly quotes, was not supplied by himself.

a 'loop-hole' for the phrenologists; but call it by what name you please, the question which concerns us is, simply, whether it is a *fact?*"

The subjoined wood-cuts will render this matter still plainer; one represents the skull of a Peruvian, the other that of Robert Burns; regarded in front, very little difference might seem to exist in the respective foreheads; estimated in profile, however, as above directed, the much greater depth of anterior cerebral lobe in the poet's head, than in that of the Peruvian's, is at once apparent.



PERUVIAN.



ROBERT BURNS.

With respect to this kind of observation, it may be well to refer to another source of mistake in judging of the forehead, which is this: there is great difference in the degree of advance which the growth of hair makes, a circumstance which, it is clear, can have nothing to do with development of the anterior lobe of brain. Yet the author has known a really low, shallow forehead, mistaken for one that was favourably developed, because the space uncovered, superiorly and posteriorly, has been considerable, giving a false idea of altitude regarded in front.

Another means of readily procuring some certain assurance that there is *something in phrenology*, and that, consequently, the subject is worthy of scientific investigation, is afforded by the observer's fixing upon such persons of his acquaintance as display remarkable facility in acquiring knowledge, and yet who at the same time exhibit an utter

inaptitude for reasoning on the knowledge attained; any one at all accustomed to study the varied phases of the human intellect, will be sure to remember some such individuals. Let the forehead, in these instances, be observed; the fullness and prominence distinguishing the inferior region, just above the superciliary ridge, or the anterior middle region, will certainly be recognised; the higher lateral portions falling back, and giving a receding appearance to the brow. Next, let persons be taken whose intellectual peculiarities are just the reverse, individuals sound-thinking and reflective, distinguished for their judgment, yet of slender acquirements, and with but little relish for details in matters of fact; here the relations also in frontal development will be reversed, in correspondence with the contrast they form in their mental characteristics; the upper division of the forehead will project, and at the least it may be expected to exhibit a perpendicular, in opposition to the rapid slope observable in the preceding instances.

A very slight acquaintance with these leading circumstances, easily observed, ought to yield abundant grounds for the conviction that phrenology is no vain conceit nor phantasm of the imagination, but that it is, more or less, a reality resting on the sure foundation of nature.

It has been said that, in the prosecution of this branch of science, as of every other, a certain preliminary capability must be gained before the student can rightly estimate and judge for himself, concerning the facts upon which it is based; in what this capability consists, has been already stated. When it has been obtained, and when the inquirer has become satisfied from *general* observation, that Gall's procedure has revealed important truths, he ought then to determine the value of the *method*, by investigating the validity of the *particular* results.

A distinction should be drawn at the very outset of the inquiry, between instances in proof, and circumstances in

mere illustration or in corroboration.[1] The average run of mankind can hardly furnish *proof* of the individual propositions, nor yet the evidence which statuary and painting supply, although they do very well for illustration, and, at the same time, supply a species of confirmation, after conviction has been realised by a study of the appropriate cases, always formed by instances of extreme development. These, in the beginning, should always afford the chief materials for observation, whether the object be to confute or to corroborate any particular point; and they are best studied, at the commencement of the inquiry, upon the living head, specially distinguished in one or two respects only. Thus, if some individual be known, whose benevo-

[1] It appears to the author, that to persons unaccustomed to exact analysis, or to close research, the distinction drawn between evidence that *proves,* and that which only *confirms,* may not at once be appreciated; and it may be said, if a truth be proved, what confirmation can it need, and what value can attach to any evidence which only supplies a superfluity? The case stands thus: all evidence comes from observation of facts; certain facts, if they have been rightly observed, *necessitate* a particular conclusion, and constitute *direct* testimony fitted to *establish* the inference to which they point. The invariable correspondence between cerebral development and speciality in the psychical characteristics, can, in reason, receive but one explanation—it necessitates the conclusion arrived at by Gall. On the other hand, there are other facts which, however accurately they may have been observed, do but *suggest* a conclusion, and which are, at the same time, compatible with some different one. Thus, all the facts which have been developed by vivisections, comparative anatomy, and pathology, and which have been placed in relation to cerebral physiology, may have been accurately stated, and yet, in the absence of other sources of knowledge, be susceptible of various interpretations. Now, in the present imperfect condition of the human mind, some doubt may reasonably be entertained with regard to any observations and facts not universally admitted; and, pending such doubts, the inference must of course remain uncertain. In this state of things, the direct testimony becomes fortified, not only by additions, but by exhibition of the harmony and analogy that subsist between it and other circumstances with which it is in some relation. As in criminal jurisprudence, *direct* evidence alone can prove incontestibly; that which is *circumstantial,* however faithful, can but corroborate the former; by itself, at the very best, it could but furnish a high probability.

lence of disposition be singular, whose every action seems prompted by love to his fellow-men; if, from childhood upwards, kindness of heart have constantly been exhibited as the predominant feeling—and who is not gifted in the power of Imitation—let the region assigned by Gall, as the cerebral seat of this disposition, be noted, and, a distinct elevation of the skull will be remarked in the corresponding part of the head, so defined in its character as to furnish a reasonable presumption, that the peripheral form of the organ is exhibited; more especially as, on multiplication of such examples, a general similarity will be observed, not only in the elevation, but also in the configuration, of the particular region. In the opposite class of cases, where the feeling of benevolence is scarcely to be recognised, let the observer seek for the organ, and assuredly a corresponding absence of all elevation will be noticed. In this way, each proposition should be investigated individually, by selection of extreme cases, positive and negative; a means whereby alone a philosophical conviction can be obtained, on one side or the other. Striking and unequivocal examples, wherein some peculiar characteristic is indisputably present, are by no means difficult of attainment; and these, when carefully observed and studied, will convince every sound-thinking and right-minded person, that phrenology, in its main particulars, is true. After the extreme cases shall have been seen to be real, no doubt whatever can arise with respect to the existence of some general law; and any difficulty which may be encountered, in afterwards dealing with cases in general experience, can never shake a conviction that has in this way been gained. *Extremis probatis media præsumantur.* By extreme instances was Gall's discovery made; by extreme instances, it must receive extension; and, by extreme instances, it must be verified, or otherwise, by all who would *scientifically* test its validity. In all other sciences, we bring forward

the strongest evidence we can obtain, as best calculated to establish new truths; and we make no unreasonable demand in requiring the same rational mode of proceeding to be followed in the study of phrenology.

Now may we advance to the particular results of Gall's method of investigation; and, in proceeding to this division of the subject, the facts and propositions shall be adduced very much in the order in which they are given in his published works. Subsequent additions, real or supposed, shall also be discussed,—not according to any arrangement fixed upon by the author, but just as for the sake of scientific regularity, they are most commonly presented to us in systematic books. It is no part of his design to exhibit the progressive steps by which Gall came to the prosecution of his peculiar method of studying the physiology of the brain; but yet it is right to notice, in this place, an idea not unfrequently entertained, that he was induced by anatomical investigations to allocate particular faculties of the mind in certain parts of the brain, and that, having done so, he appealed to nature for corroboration of his ingenuity. Nothing can be a greater mistake, as will be further and most abundantly illustrated in the ensuing pages. When a writer so accomplished and well-informed as Dr Bostock[1] falls into such an error of assertion, it becomes proper to direct attention to this circumstance.

By comparing development with manifestation, Gall was led to ascertain, in the following way, the encephalic organ of the *sexual instinct*, usually denominated

AMATIVENESS. Dr Gall was the medical attendant of a young widow, who, shortly after the death of her husband, was seized with melancholy and with hysterical convulsions. These attacks were preceded by a sense of tension and

[1] Vide Physiology, vol. iii. p. 264.

heat about the nape of the neck; in a few moments afterwards, she would fall to the earth in a state of rigidity, the neck and vertebral column being violently dragged backwards. "La crise ne manquait jamais de se terminer par une evacuation qui avait lieu avec les tressaillements de la volupté, et dans une véritable extase; après quoi, elle restait sans attaques pendant quelque temps."[1] During these paroxysms, Gall had several times occasion to support the neck; and, in doing so, remarked a very considerable prominence at the nape, between the two mastoid processes of the occipital bone. This circumstance arrested his attention; it was exactly one of those incidents which ordinarily originated some primitive suggestion from which, as a starting point, he followed out an investigation. It occurred to him that a connexion might exist between the erotic disposition of the patient, and the fulness in the neck; more especially as he learnt, shortly afterwards, from her own acknowledgments, that, from infancy upwards, the predominant inclinations had always corresponded with what was now so distressingly obvious. Hereupon, he interrogated nature upon a large scale, multiplied observations, and procured abundant confirmation and realization of the idea which he had previously formed; always discovering a fulness in the basilar region of the occiput, whenever he had moral certainty that the sexual instinct was powerful, and a receding, contracted nape, when he had reason to feel assured that the passion was but weak. Connecting these observations with his anatomical knowledge, he determined the cerebellum to be the organ of the propensity in question; because, according to the development of this structure, there is a degree of amplitude and fulness, greater or less, in the space marked out by the occipital ridge.

In this way, Gall was led to the discovery that the cere-

[1] Gall. Sur les fonctions du Cerveau. Vol. iii. p. 246.

bellum is the organ of *amativeness*. Proof may be obtained of reality in the discovery, by repeating the observations; and it is only by making counter observations, that any just attempt can be undertaken in disproof. If the facts have been accurately observed, by reiterated researches they can but be confirmed; and no seeming difficulties encountered in vivisections, the study of the animal kingdom, or in disease, can weaken the conclusion to which they fairly and directly lead. "If every theory," says M. Bouillaud, "found in contradiction with one rightly observed fact be false, so every fact in contradiction with a theory *rigorously demonstrated* has been badly observed."

In the repetition of Gall's observations upon the cerebellum, let the inquirer, pursuing the directions given above with regard to the necessity of taking extreme cases, first familiarise himself with the outward indications of size in the cerebellum. The woodcuts below will exemplify the external appearances presented under different conditions of magnitude.

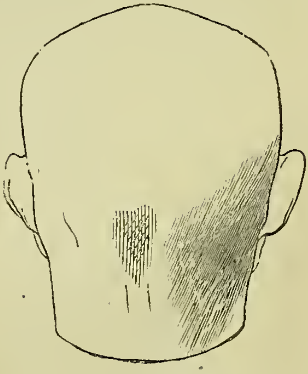
Fig. 1.

REV. MR M.

Cerebellum moderate.

Fig. 2.

LINN.

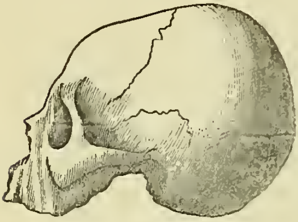


Cerebellum large.

As the volume of the cerebellum is indicated by the extension of the inferior surface of the occipital bone backwards and downwards, or by the thickness of the neck at these parts, between the ears, so the difference between a moderate and large development will be appreciated by observing the thickness of the top of the neck in the two figures.

The annexed cuts represent, in profile, three skulls illustrating different degrees of size in the cerebellum, as indicated externally.

Fig. 3.



Cerebellum large.

Fig. 4.

Cerebellum middling.

In Fig 3, O represents a large development downwards; backwards also, the extension is considerable; in Fig. 4, the distance between M, the mastoid process, and S, the spine of the transverse ridge of the occipital bone, is large, although the swellings O, do not droop as in the preceding skull; in Fig. 5, the cerebellum is small, and it will be seen that the base of the occipital bone extends only a short distance backwards from the mastoid process, while the occipital fulness downwards is not manifest as in Fig. 3.

Fig. 5.

Cerebellum small.

To the correctness of this mode of estimating the volume

of the cerebellum, no sound objection will apply. Dr Todd and Mr Bowman, however, in their recent work, observe that " a thick neck and large occipital region may, and probably do, indicate a large mesocephale more frequently than a large cerebellum." Now, the *mesocephale*, so called, can only be expected to influence the development of the basilar portion of the occipital bone by agency of the *pons Varolii*—the commissure of the cerebellum—which enters so largely into the composition of the structure in question; and, as Dr Todd and Mr Bowman state in the same work, " the fibres of the pons are always developed in the direct ratio of the hemispheres of the cerebellum," it is difficult to see in what way their objection is intended to apply. If the mesocephale be large, so is the cerebellum; and if the " thick neck and large occipital region do indicate" the former fact, how can it exist more frequently than the latter, to any extent, if " the fibres of the pons are *always* developed" in a corresponding ratio? There is not even plausibility in this objection.

In the early observations conducing to the establishment of any point, Gall first ascertained the coincident existence of the two sets of conditions, and the reciprocal dependence which they appeared to hold; and, simply noting the undue tendency to particular actions associated with a correspondent development of brain, he named the cerebral portion the organ, sometimes of the tendency in question, and sometimes of the conduct to which it led. Thus, *Amativeness* was designated the *Instinct of propagation;* and, in reference to other cerebral organs, he spoke of an organ of *Theft*, of *Cunning*, and so on; mainly intending, by this course, to keep clear of all premature speculation, and to retain his disciples within the limits of demonstration. Nevertheless, it is a grievous error to suppose, that Gall was not well aware of the distinction to be made between the fundamental powers and dispositions, and the actions to which he regarded these latter as mainly contri-

buting. Indeed, he reasoned most powerfully, perspicuously, and successfully, upon such matters, and proposed to decide upon the speciality of any instinct, inclination, sentiment, or talent, by determination of the existence, or otherwise, of the following conditions:—*First*, Its presence in one kind of animal and not in another; *Secondly*, Its systematic variation in the sexes of the same species; *Thirdly*, The absence of all proportion in its energy to the other faculties of the same individual; *Fourthly*, Its manifestation and decay occurring, earlier or later, irrespective of the other powers; *Fifthly*, Its capability of action or repose, singly; and, *Sixthly*, Its separate suspension or deterioration by disease. When, moreover, he had traced the mutual relation subsisting between some special faculty (shown to be such by correspondence with the conditions just enumerated) and some particular cerebral region, he would finally advance a proposition as *established;* just as we do in the case of the senses and their nerves. The specific differences in the functions of sight, hearing, smell, taste, and touch, and the fact of division in their respective organs, are, in fact, maintained by an identical course of reasoning, and substantiated by a like process of demonstration.

In this way did Gall determine the speciality of the sexual instinct, and its organic dependence upon the cerebellum; a truth which, thoroughly studied, cannot be doubted. It has not only been attested by the direct observations of Gall himself, as recorded in his works, but it has been abundantly confirmed by those of Spurzheim, Combe, and many others. The proposition has been strengthened by the results of many mutilations, some of which have been recounted by Gall, Vimont, and Larrey; facts in natural history and comparative anatomy have been extensively contributed by the discoverer, and more especially by Vimont, in further corroboration; and the phenomena of disease have largely been adduced, more particularly by Spurzheim, Serres, and Andrew

Combe, to confirm the doctrine in question; altogether, constituting such a mass of accumulated testimony as rarely is equalled, and certainly never surpassed, in the instance of any analogous proposition. The author is well aware that other functions have been assigned to the cerebellum by many able physiologists, though few have ventured to controvert, absolutely, its association with the amative propensity. Without actually *denying* the accuracy of the notion which locates some special influence over the muscles to the cerebellum, the writer dares to affirm that the assumption is purely *gratuitous*, and that it has not one tittle of conclusive evidence upon which to rest. This position he shall seek to maintain in succeeding chapters, when discussing the statements made by vivisectors, comparative anatomists, and pathologists, in support of the muscular function of the cerebellum; he expects to be able to show, not only that the facts stated are inadequate to prove the doctrine, in support of which they are adduced, but that they are in perfect harmony with that which appropriates the whole cerebellum to the function assigned to it by Gall.

Dr Todd and Mr Bowman have been several times referred to in the present treatise; their work on the Physiological Anatomy and Physiology of Man possesses many and merited recommendations; and, as it is among the most recent books discussing such subjects as the present one, the author thinks it desirable to cite, and briefly to remark upon, certain reasons which they have advanced, formally and in detail, in opposition to the physiology of the cerebellum, as taught by Gall; reasons which, in some quarters, would appear to be regarded as possessed of *force*, since, in one of the journals (the "Provincial Medical)," they were quoted as the choice specimen of the work.

Objection.—" 1. It is extremely questionable how far the sexual instinct admits of being separated from the emo-

tions,—from those especially which are clearly instinctive in their nature; and, even if it were separable from them, it seems scarcely of such importance, when compared with other instincts, as to need a separate organ of great magnitude and of complex structure. If we compare it, for example, with the instinct of self-preservation, as manifested in providing, either for the wants of the body, or for defence against assault, it certainly cannot be admitted to have a superior influence in the animal economy to this, the most pressing of all. Yet it is not pretended to assign a separate seat even to this."

Remarks.—The preceding objections seem resolvable into three distinct propositions: *first*, That no adequate proof exists of speciality in the feeling in question; *secondly*, That its importance in the animal economy is too inconsiderable to demand a large subservient organ; and, *thirdly*, That a more powerful instinct, that of self-preservation, has no distinct cerebral apparatus assigned to it. Now, as regards the first proposition, what do these writers deem to be evidence of distinctness in the character of any instinct? The conditions of speciality expounded by Gall, and briefly adduced in a foregoing page, are all present in the case of the generative feeling. Would these be objected to by Dr Todd and Mr Bowman? If so, can they propose any reasonable canons of their own, which will not, in their application to the sexual feeling, result in a like conclusion? By every rule determining the distinctness of any of the special senses, the instinct in question must be deemed fundamental and primitive. This will easily be ascertained by application of any test to the latter, which demonstrates the fact in the case of the former. Let Dr Todd and Mr Bowman try the experiment. Indeed, the first proposition in objection is mere unsupported assertion. Then, as regards the next, what do these physiologists consider to constitute a measure of importance in the economy of life, in assigning only a

secondary, comparatively insignificant place to the feeling now under discussion? No explanation upon this point is afforded. If, however, the design or final cause of any inward impulse is to pass for something in an estimate of its importance, what can possibly be *more* important than the continuation of the species, in obedience to the first and most universal injunction, "*Increase and multiply?*" If we do but reflect, for one moment, upon the influence which the sexual passion has exercised upon individuals, families, society, or nations, who shall maintain with justice that other feelings have evinced, in their effects individually, a greater extent of operation? Witness the disastrous consequences of its irregular and depraved manifestations! Ruin to health, loss of fortune, blighted character, temporal and eternal ruin to individuals; the tenderest ties violated, hearths made desolate, and utter destruction to all peace and happiness, the occasional results in families; society vitiated to the heart's core, and the seeds of its dissolution sown in the perpetration of deeds at which nature is aghast; and, as respects nations, wars the most distracting, bloody, and desolating, have had their origin traceable to the lustful vices of potentates. And, if we contemplate the opposite picture, who shall over-estimate the advantages to virtue and happiness in individuals and in the world, flowing from a right direction of the disposition, or the feeling, which produces marriage? What *can* Dr Todd and Mr Bowman have been thinking about, in declaring this psychical quality to be of but little "importance when compared with the other instincts?" In the third proposition which the objection includes, an assertion is hazarded which would have been avoided, had a more complete knowledge of the subject been possessed. It is affirmed, in regard to the instinct of self-preservation, that "it is not pretended to assign a separate seat to this." And yet, whether the instinct of self-preservation have reference to "providing for the wants of the body, or for defence against assault," there

is a separate seat in each case, at least *pretended* to be assigned; and if mere love of life be principally signified, there is at any rate *pretence* even here. Dr Spurzheim, without any assumed discovery upon the subject, conceived that, for such a feeling, a special organ must exist. Dr A. Combe has recorded an observation suggestive of its seat; and Dr Vimont thinks that he has demonstration upon the subject. It is truly astonishing to see the groundless complacency with which some parties conceive themselves entitled to dispose of the physiology of Gall, by the loosest and most random assertion. It has long been so popular a vocation to write against phrenology, that even able and sensible men do not hesitate unthinkingly to publish nonsense, by way of objections against it, which they would unhesitatingly reject if addressed to their own understandings on a serious subject by other authors. It is only in this way that we can account for the appearance, in some works of merit, of arguments against Gall's doctrine, which are wholly unworthy of the sources from which they proceed.

Objection.—" 2. The nature of the generative instinct is scarcely such as to require, in its central organ, connexions so extensive as those possessed by the cerebellum. It is not likely that this organ would be connected with any other part of the spinal cord than that from which nerves are derived to the organs of generation; nor is it conceivable that an instinct like this should require for its exercise fibrous matter in such large quantity as it exists in the cerebellum, taking its rise from so great a surface of vesicular matter."

Remarks.—It is a fundamental maxim in science, that neither a *fact*, nor the opposition to a fact, should be allowed to *rest upon an argument*, when it can be determined by observation; yet this is an example of attempting to set aside an alleged fact by mere argument on its probability. This objection, moreover, is supported neither

by fact nor by argument; it is a series of assertions, based only on the sense of analogy, as it obtains in the minds of the objecting parties. "It is not likely—nor is it conceivable," constitute the entire foundation. May it not justly be replied: It *is* likely and it *is* conceivable—when the subject is regarded in the whole of its bearings?

Objection.—"3. The generative instinct is not so pre-eminently developed in man, as to account for the great superiority in size, as well as structure, of the human cerebellum over that of the lower animals, even of the mammiferous class. On the contrary, it may be safely asserted that this instinct is much more powerful in the monkeys, and also in the frogs; in the latter of which the cerebellum is absolutely very small, and especially so, relatively to the spinal cord and the cerebral lobes."

Remarks.—Unsupported assertion, once more; the first proposition is neither proved, nor justly illustrated, by what is affirmed in the second, respecting the monkey and the frog; the writer shall return to this subject in the sequel.[1]

Objection.—"4. If the cerebellum be the seat of the generative instinct, it ought to exhibit marked indications of wasting, in cases where the genital organs have been mutilated; or where they have decayed in the natural progress of age. Yet the recorded cases of this nature are by no means conclusive; on the contrary, M. Leuret's remarkable observations show, that, in the gelding, the cerebellum is actually heavier than in either the stallion or the mare."

Remarks.—That which Dr Todd and Mr Bowman say "ought" to happen, if the cerebellum be the seat of the generative instinct, *has* happened; the recorded cases of this kind are as complete and satisfactory as other analogous facts; and before they are pronounced to be "by no means conclusive," reasons ought surely to have

[1] Vide chapter ix.

been given. M. Leuret's statements will receive attention in the course of the ensuing pages.[1]

Objection.—" 5. It does not appear, from pathological research, that the cerebellum has any particular influence upon the genital organs. Injury or disease of that organ very rarely produces any effect upon the penis; but lesion of the medulla oblongata or the spinal cord is very apt to occasion a semi-erection of that organ."

Remarks.—It appears, on the contrary, from pathological research, that the cerebellum *has* some peculiar influence upon the genital organs. Injury or disease of that organ very often produces an effect upon the penis.[2] Whether lesion of the medulla oblongata or the spinal cord be, or be not, very apt to occasion a semi-erection of that organ, does not immediately relate to the present question.

LOVE OF OFFSPRING. For several years before Gall was led to connect this feeling with any distinct portion of the brain, he was struck with an almost constant variation in the skulls of the two sexes; he noticed that, in general, there was a much larger mass of brain projecting in the direction of the higher portion of the occiput in women than in men, indicated by a fulness in this region, and greater distance from the external opening of the ear. He strove, for a long time, to think of some quality which was remarkable in women in comparison with men, with which he might possibly be able to associate this cerebral peculiarity. His efforts for a long time were vain. At length, he observed that the cranium of monkeys presented a marked resemblance to that of women, in the peculiarity before mentioned. From this circumstance, he assumed it to be probable, that the portion of brain behind the prominence in question was the encephalic organ of some faculty or quality which women and monkeys possessed, alike, in a high degree. But it was some time before he

[1] Vide chapter viii. [2] Ibid.

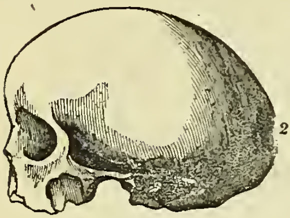
hit upon the right route; in a favourable moment, however, he was struck with the extreme affection which monkeys notoriously bear to their young. Hereupon, he proceeded to contrast all the male and female skulls of every species, in his collection, and discovered a general correspondence in the same respect; the occipital region being developed, for the most part, to a greater degree in the female crania of all animals than in those of the males, just as he had previously noticed the fact to be in the human species. In this way, Gall attained the primitive *suggestion;* the *proof* was obtained, as in all other cases, by taking individuals of the same species, remarkable for their opposition in the degree in which this feeling was manifested, and observing that the development of the region, *presumed* to be the seat of the instinct, always corresponded, under just circumstances of comparison, with the extent of power in the manifestations. On propounding to his disciples the fact which he considered himself to have established, he appealed largely to the animal kingdom and to the phenomena of disease, for corroboration and illustration. His own works, those of Spurzheim, and Vimont, the publications of Dr and Mr Combe, and the Edinburgh Phrenological Journal, abound, not only in direct evidence of the existence and cerebral locality of this feeling, but also in the subordinate proofs which indirect sources afford. The testimony seems complete.

It has sometimes been alleged, that Gall discovered the organ of Love of Offspring, or *Philoprogenitiveness*, as it is technically denominated, primarily by comparative anatomy; a circumstance which, if true, would invalidate much of the reasoning that has gone before. It is a mistake, however. The *first* notation of difference in the cerebral development of the two sexes was made upon the human head; a correspondence was afterwards observed in animal heads, *confirmatory* of the previous notion; a *hint* was then obtained by remembering a certain peculiarity in the

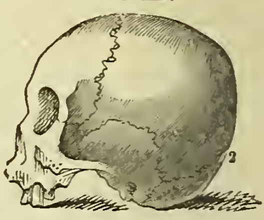
monkey tribe; the *experimentum crucis* was made upon the human species; and thus was the *proof* gained, corroborated by comparative anatomy, natural history, and other collateral branches of knowledge,—the whole proceeding having been rigorously in conformity with the philosophy set forth in the present work.

The figures below illustrate the aspect of the cranial conformation in extreme instances, the organ (2) being large in the girl and small in the Peruvian.

GIRL.



PERUVIAN.



Let any one seeking either for conviction or for facts in confutation, examine the heads of separate individuals, distinguished for their opposition in the present particular; it will in that case be seen that, where there is a *passion* for children, the occiput has some resemblance to that of the girl in the woodcut, and that where there is comparative *indifference*, the configuration will more nearly resemble the Peruvian's head. If, after sufficiently extensive and accurate observation, this do not appear to be the case, Gall's physiology *pro tanto* is disproved; and inasmuch as phrenological observers all coincide in ranking the organ of *philoprogenitiveness* in the best established category, disproof in this one particular would certainly *shake* the presumption in favour of the entire doctrine. In no other way, however, than by accumulation and presentation of direct facts faithfully observed, can the proposition be justly controverted; nor in any other way, will a conviction of its truth be

attained, of a character that is firm, and to be permanently relied upon. But when once the mind has been *philosophically* satisfied, by study of the extreme cases, middling ones will supply yet further evidence; painting and statuary will afford illustration: and so will departments of knowledge but incidentally related to inquiries of this nature.

ATTACHMENT. Dr Gall was requested to mould for his collection the head of a lady, who, he was told, was a model of friendship. He states that he did so, rather from a wish to oblige, than in the expectation of making a discovery. In examining this head, he observed two decided prominences, one on each side, and slightly above the organ of philoprogenitiveness. Never having before noticed projections in this region, and knowing from their locality, and other circumstances, that they must be occasioned by development of the subjacent convolutions of brain, he supposed it to be likely that the prominences in question might be an outward indication of some special faculty or disposition of mind. Hereupon, he procured all possible information concerning the character of the lady, both by inquiry and by personal observation. Every thing concurred to exhibit her as maintaining, under all circumstances, a firm and inviolable attachment to her friends. Although her fortune had experienced great changes, at different epochs, and although, at several times, she had passed from poverty to high station, the sentiments which she ever maintained towards friends were unchangeable. This circumstance gave rise to reflection; the idea arose in the mind of Gall, that this disposition of attachment might very likely be fundamental, in association with the cerebral region, so conspicuous in her head. He observed extensively, and arrived at the conclusion that this was actually the case. Dr Spurzheim, Mr Combe, and others, have so frequently repeated Gall's observations in this particular, with the same results, that no rational doubt upon the subject can exist in the minds

of those who shall interrogate nature for themselves. The author has done so, and in such a way obtained a conviction, which no supposed analogies or speculations can or ought to remove. Observation upon this point is not difficult; *strong friendly feeling* is readily discriminated, and the corresponding development in extreme cases is easily appreciated: let the two conditions be compared, and the result will be seen. If the experiment be made in this manner, by examination of remarkable cases, both positive and negative, conviction will surely come. Afterwards, do as Gall did, extend and elucidate the knowledge you have thus gained, by study of natural history, comparative anatomy, or moral philosophy; seek for illustrations in the ordinary actions of mankind. In criticism of statuary and painting, you may avail yourself of an acquaintance with this physiological fact, and, in this way, *strengthen* your convictions. Do not, however, let them rest fundamentally on any of these latter grounds. The situation of the organ of Attachment precludes all satisfactory illustration of the development by woodcut.

COMBATIVENESS. Dr Gall observes that, in most cases, it is much more easy to discover the organ which mainly determines a certain conduct, than the fundamental faculty or disposition itself. Actions, the consequences of *extraordinary* activity in an organ, strike the attention much more readily than its primitive destination, or even than the conduct which *ordinarily* flows from its influence. For this reason, Gall was led, in the great majority of instances, to *discover* through manifestations of unwonted activity. The leading peculiarity, and the speciality of some faculty or disposition, being in this way, through actions, once fairly recognised, its primitive destination may gradually become a matter of reasonable inference. The history of the discovery of Combativeness elucidates these remarks.

Feeling uncertain how far language afforded suitable expressions for designating all the fundamental faculties

and qualities, Gall was anxious to see in what way the common people discriminated various manifestations of character, and how they expressed themselves. He assembled in his house a certain number of individuals, taken from the lowest classes of society, and of different occupations—coach-drivers, porters, and servants. He acquired their confidence, and disposed them to frankness, by distributing to them money, wine, and beer. When he found them in a favourable disposition of mind, he made them tell him all that they knew of one another, good or bad, relating to their several dispositions.

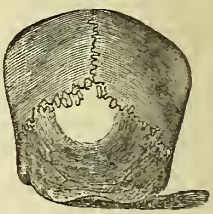
In the revelations which followed, most prominence was given to the quarrelsome traits of character; the disputatious and pugnacious tribe delighted to recount their valorous exploits, dwelling with contempt upon the conduct of some of the others, their pacific associates, whom they contemptuously denominated *poltroons*. Gall was curious to see if there was any thing in the heads of the one class distinguishing them from those of the other. Accordingly, he placed the quarrelsome persons all in a row, and did the same thing with the pacific ones; then he proceeded to a careful inspection of their heads. He observed that, uniformly, the heads of the pugnacious class were largely developed in the region immediately behind, and on a level with, the upper portion of the ears; and that, in the opposite class, a comparative deficiency existed in this respect. The researches being continued, confirmation of these observations was obtained. Gall himself designated, in varying phraseology, the inward characteristic whose outward sign he thus detected: he called the portion of brain occasioning this external indication, the organ of the *Instinct of Self-Defence*, of the *Disposition for Contention*, of *Courage*. The term *Combativeness* is now most commonly employed; and, however open to objection, it probably expresses the meaning better than any other single word; which meaning may best be inferred by study of the associated class

of actions. It would be unnecessary repetition to say in what manner Gall procured subsidiary proofs and illustrations. The two skulls represented below, exhibit the requirements for primary observation—cases of extreme development, and of deficiency—of the organ of Combativeness, the situation of which is indicated by the figure 5.

GENERAL WURMSER.

Large.

CINGALESE BOY.



Small.

DESTRUCTIVENESS. In comparing the crania of animals, Dr Gall discovered a characteristic difference between those subsisting on fruits and those consuming flesh. He placed on a table, horizontally, a number of crania of herbivorous animals, and, raising a perpendicular line from the external opening of the ear, he found that there was placed behind this line but a small portion of the posterior cerebral lobes, in addition to the cerebellum. In repeating this experiment with the skulls of carnivorous animals, he ascertained that a considerably larger mass is situated there. He found also that the skulls of the latter were more prominent above the ear than those of the former. For some time, he made no special application of these observations to his particular physiology. He contented himself with pointing out to his auditors, in the delivery of his lectures, that, by inspecting the cranium, even in the absence of the teeth, it was possible to distinguish whether an animal belonged to the graminivorous, or the carnivorous classes. At length the skull of a parricide was brought to him; he put it aside, never imagining that the

heads of murderers would aid him in his researches. Shortly afterwards, he received the skull of a highwayman who, not satisfied with robbery, had assassinated several persons. He placed the two crania in juxtaposition, and frequently examined them. Every time that this was done, he was struck with the circumstance, that, however different their configuration was in every other respect, both skulls were alike in this: they had each a prominence fully developed, immediately above the external opening of the ear; but then he discovered the same prominence in certain other crania in his collection; still he could not resist the idea, that there was something more than mere chance in the coincident development of the two murderers. He reverted to his previous observation regarding the difference to be noticed between the skulls of graminivorous and carnivorous animals, and he conceived it possible that some connexion might exist between the carnivorous instinct in animals, and the homicidal tendency in man. He resumed his examination of human heads, and very soon procured evidence that the cerebral parts lying beneath the before-mentioned region of the skull, are instrumental in the manifestation of a feeling which, in its abuse, leads to *cruelty*, and, where predominant, as an ill-guided passion, hurries, occasionally, to the perpetration of murder. Dr Gall named the psychical quality, the *propensity to kill.* It is an error to suppose that he spoke of an organ of *murder*, in the English sense of the word. *Killing*, in certain cases, being a necessary operation, as in procuring adequate supplies of food, he regarded this as one of the legitimate aims of the disposition in question; but, "I have never," says he, "in speaking of the *instinct du meurtre*, meant a propensity to homicide." The word *destructiveness*, employed by Dr Spurzheim, expresses very nearly the meaning of this propensity, abstractedly considered. Mr Combe's System of Phrenology contains some striking illustrations of this feeling,

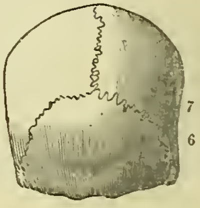
as displaying itself under every variety of circumstances; to these the author would refer, as it is no part of his own plan to discuss these details in any extended point of view.

The casual reader may imagine, from the account just given of the discovery of the organ in question, that, here at least, comparative anatomy constituted the primary source. Yet the contrary is the fact. Whilst Gall studied and compared the crania of different animals, without guidance from human physiology, no advance whatever was made towards a just interpretation of the facts he saw; it was after he had obtained the skulls of the two murderers, and noticed their points of coincidence, that he seized upon the primitive idea. In the further progress of his reflections and observations, hints were gathered from comparative anatomy, natural history, and disease, but the first notion, and the subsequent proofs, were obtained by comparing development and manifestation in the human species; then, as was his wont, he drew largely from other departments to illustrate, and to corroborate, the previous induction.

The subjoined figure, on the left, represents the skull of Tardy, who was a bloody pirate, and in whom the organ of destructiveness (6) is very large; in contrast with which, on the right, is again placed a Cingalese, a tribe remarkable for aversion to the destruction of life.

TARDY.

CINGALESE.



In the Appendix will be found a brief account of some

of the facts, direct and collateral, by which the organ of Destructiveness is established. It is given as an example of the evidence upon which the functions of the cerebral organs are affirmed.

SECRETIVENESS: The *disposition to conceal*; the essential element in *cunning*. Dr Gall, in early youth, was struck with the character and form of the head of one of his companions, whose disposition was generally good, but who delighted in cunning and finesse. Although a faithful friend, he experienced a singular pleasure in making game of his school-fellows, and in mistifying them. His expression of countenance and general manner were remarkably indicative of cunning. The head of this person was very large above the temples. At a later period, he had another acquaintance who, at first sight, appeared to be candour itself; he was distrusted by no one. Yet his gait was suspicious, his deportment, in some respects, like that of a cat seeking to ensnare a mouse; he was, in fact, false and perfidious, deceiving friends, instructors, and parents alike. He had a like configuration of head. Gall had a patient who died of consumption, and who, during life, had generally passed for an honest man. After his death, Gall was struck with the breadth of his head in the temporal region; a little afterwards he ascertained that he had been a thorough cheat. At Vienna, he became acquainted with a physician of considerable attainments, who, however, was very generally despised, on account of his reputation as a trickster. He often declared to Gall, with a serious air, that he knew no pleasure equal to that of making dupes; and especially in deceiving persons the most distrustful of him. As this physician had a similar development of head, he naturally fell upon the idea that the sentiment, or disposition, mainly inducing *cunning*, was located in a particular cerebral organ, corresponding with the cranial prominences he had noticed. Repeated observations, on his own part, and that of others, leave it in the

highest degree probable that the chief ingredient in a cunning disposition, — the propensity to conceal,—is a primitive feeling associated, as conceived by Gall. Mr Combe regards this point as established; the present writer can scarcely vouch for so much, from his own experience. The Hindoo skull, here represented, exhibits a large development in the region in question (7).

HINDOO.

ACQUISITIVENESS. When Gall was in the habit of collecting in his house people of the lower classes of society, and engaging them to tell tales of one another, for the purpose previously stated, a considerable number accused their fellows of petty thefts, and delighted in pointing out such as excelled in this respect. These, moreover, put themselves prominently forward, as if proud of their *savoir faire.* Others, on the contrary, expressed the greatest horror at any approach to theft, preferring to suffer from hunger, rather than to accept any share in the booty of their comrades. These were ridiculed for their conduct and stigmatised as fools.

When Gall had got together a considerable number of instances, he divided them into three groups. The *first* included the thievishly disposed persons; the *second*, those who shrunk from the very idea of theft; and *third*, those who seemed indifferent in this particular. He studied the cranial peculiarities of these three classes of persons, and found that, constantly in the first, a long prominence was observable, extending from the last mentioned organ, almost as far as the external angle of the superciliary ridge; and that, on the contrary, in the class abhorring all thought of robbery, there was a flatness in the same region. With the group remarkable in neither respect, the part was undistinguished, varying in the individual cases, but to no very great extent. On repeating this kind of experi-

ment with other assemblages, the same results uniformly occurred.

Under these circumstances, the idea was conceived by Gall, that the propensity and the development stood in some fixed relation to one another. Great numbers of facts, recorded both by himself and others, leave no reasonable doubt upon this subject; and any one who shall go to nature to judge for himself, may readily obtain decisive evidence of this point. The general conclusion from the whole of the facts seems to be, that the sentiment of *property*, the disposition to *acquire*, is a fundamental quality of the mind, having its cerebral organ situated as before described; and that, in its abuse, it leads to *avarice;* and, unrestrained by other and higher motives, to *theft.* The situation of the organ, and its appearance when largely developed, are shown in the subjoined wood-cut (8).

OLD MISER.

SELF-ESTEEM. This feeling, as associated with a special cerebral organ, was ascertained by Gall in the following manner :—He was struck with the superior manners of a certain beggar, wondering how such a person could have

been reduced to mendicity. He took a cast of his head, and discovered, on examining it with attention, a projection extending from above downwards, on the upper and back part of the middle line, which could but arise from the development of the subjacent cerebral parts. Not having noticed this prominence particularly in other heads, he was anxious to make out what it indicated. After many questions directed to the mendicant, with a view to discover the salient points of character, Gall requested him to relate his history. The beggar stated that he was the son of a rich merchant, from whom he had inherited a considerable fortune, but that he had always been too proud to apply to business, either for the preservation of his inheritance, or for the acquirement of a new fortune, and that this unhappy pride was the sole cause of his wretchedness. Dr Gall made several remarks to him, showing that he doubted his veracity; but the beggar always reverted to his pride, and seriously stated that, even now, he could not bring himself to follow any kind of work. By this person's reiterated assurances, Gall was led to think of pride as a fundamental sentiment, and to observe other heads, wherein the particular development remarked in the present instance, was conspicuous; and the conclusion attained was the complete establishment of the relation subsisting between the feeling of pride and the cerebral part noticed in excess in the case of the mendicant. Of the reality of this discovery, no one who consults nature can doubt.

Unusual facilities exist, in the present instance, for determining the accuracy of Gall's observations. Little argument or illustration is needed to show the innate character of pride, or its complete independence, in many cases, of all outward circumstances. No sentiment or state of mind is more easily identified. The cranial form, moreover, which indicates excess or deficiency of the cerebral organ, is very readily recognised and appreciated. If you do not think the proof of Gall's doctrine, in this particular, unworthy the

very little trouble the experiment will cost you, select two of your proudest friends. Let these have displayed a sense of self-importance under all sorts of circumstances—you will not mistake foolish ebullitions of vanity for the well-marked indications of self-esteem—see the sedate step, the unrelaxed features, the high uplifted head. When you shall have realised these examples, take two of your meekest friends—persons with no self-reliance; whose every action and gesture shall evince a degree of modesty excessive, and at times even painful to contemplate: every body meets with such instances. Regard the heads of these different characters; look at them in profile; see if the crown, commonly so called, do not rise high above the opening of the ear in the examples of pride; compare its elevation in these with what you find in the other cases, where you will notice, in the corresponding region, a relative depression. The respective examples, in each extreme, you will find to resemble very much the subjoined outlines, the figure 10 pointing out the exact locality of the organ.

F. CORDONNIER.

Self-Esteem moderate.

MR A——.

Self-Esteem large.

If, moreover, in the proud instances selected, the hand

be placed on the crown of the head, a fulness and prominence will be disclosed, and a comparative flatness in the opposite cases. No *observer*, after such appeals, will doubt regarding the reality of discovery in the case of this portion of the encephalon.

Love of Approbation. Whilst Gall was engaged in verifying his discovery concerning the organ of Self-Esteem, he encountered, in a lunatic asylum, a woman who imagined herself to be the Queen of France. He looked for a large organ of Self-Esteem; but, instead of the oval prominence at the superior posterior region of the head, which he expected to find, he perceived a very sensible depression, and, on each side of this, a large round projection. He was at first greatly embarrassed by this circumstance. However, he soon perceived that the kind of madness characterising this instance, differed altogether from that which seemed to depend upon, or to be associated with, undue pride, as he had noticed it in men. These latter, under the said circumstances, are serious, calm, imperious, lofty, and arrogant, affecting a rigorous air of majesty. Even in the most marked paroxysms, their every movement, every expression, carries the imprint of the feeling of power and rule which they believe themselves to exercise over others. With persons mad from vanity, on the contrary, all this is quite different. There is displayed a restless frivolity, and unceasing babble, the most affected forwardness, eagerness to set forth their supposed high birth, and inexhaustible riches, promises of honour and favour; in a word, a ridiculous mixture of folly and absurdity.

"The *proud* man," says Gall, "is penetrated with a sense of his superior merit, and from the summit of his grandeur treats all other mortals either with indifference or contempt. The *vain* man attaches the greatest importance to the opinions of others, and seeks their approbation with eager-

ness. The *proud* man expects that his merit shall be sought out; the *vain* man knocks at every door in order to fasten attention upon himself, and he supplicates for the smallest honour. The *proud* man disdains the marks of distinction which constitute a source of happiness to the *vain* man. The *proud* man revolts at foolish eulogiums; the *vain* man inhales with delight the incense of applause, however absurdly and unskilfully administered."

The justice of this distinction will be readily admitted by all to whom the study of character has been a habit. Repeated observations revealed to Gall the distinctness of Love of Approbation and Self-Esteem, and the existence of separate cerebral organs; that of the former being, curiously enough, on each side that of the latter; from which circumstance, many may have supposed that his location of the organ was consequent upon the distinctions between pride and vanity, above drawn. It was not so, however; the primitive idea was seized upon, as just related, from observation, and the reasoning was all *à posteriori*.

Owing to the situation of the organ, its greater or less development cannot be very well shown by illustrations, as in the case of most of the others. It is one, however, very easy of recognition; and its size, in extreme cases, is appreciable at once by the merest novice. It is clearly established.

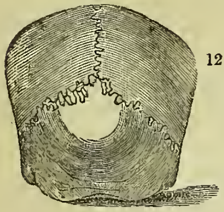
Cautiousness. At Vienna, Gall was acquainted with a prelate, a man of excellent sense and considerable intellect, who nevertheless incurred the dislike of many persons, because, from fear of compromising himself, he infused into his discourses reflections interminable, and delivered them with an insupportable slowness. When any one began a conversation with him, it was very difficult to come to a conclusion. In the midst of his sentences, he would perpetually pause, repeating the commencement of

them two or three times before proceeding. Gall's patience was often severely tried on such occasions. He never in his life gave himself freely up to the current of his thoughts. He recurred a hundred times to what he had said, as if consulting with himself whether an amendment could not be made in some point. His actions were just like his speech. He prepared with infinite precautions for undertakings the most insignificant. He submitted every connexion, before it was formed, to the most rigorous calculation. This particular instance, however, did not, singly, arrest his attention.

This prelate happened to be connected, in some public business, with a councillor of the regency, whose habitual irresolution had procured for him the *sobriquet* of *Cacadubio*. At certain examinations which took place in the public schools, these two individuals were placed one beside the other, and Gall had a seat immediately behind them: this circumstance furnished him with the opportunity of examining their heads. What struck him in both cases was, that the head was very broad in the superior-posterior-lateral region. This extraordinary breadth coinciding with the peculiarity of character in the two men, differing, at the same time, as they did, in nearly every other respect, suggested that their irresolution and ridiculous caution might be the manifestation of some primitive feeling, in connexion with the cerebral portion inducing the remarkable conformation of the head just noticed. Repeated observations, both by Gall and others, have established beyond all doubt an organ of *Cautiousness*, the situation of which is beneath the central points of ossification in the parietal bones. When the brain in that region is developed in excess, it gives great breadth to the head in the manner observed in the foregoing experience of the discoverer.

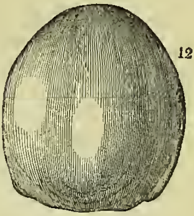
The woodcuts below represent its appearance when large and small; its excess or deficiency is very easy of recognition; fig. 12 points out its exact locality.

CINGALESE BOY.



Large.

GIRL.



Small.

INDIVIDUALITY and EVENTUALITY. By the faculty of *Individuality*, the mind is considered to become acquainted with objects simply as *existences;* and by that of *Eventuality*, with *changes in their relations*—a distinction not made by Gall, whose observations merely led him to associate the capacity for knowledge with the middle of the lower part of the forehead; he named the cerebral part as one organ—that of the "sense of things." Gall says, that after he had ascertained the existence of an outward sign of the talent for learning by heart, he soon saw that it did not indicate all kinds of memory. He observed that, among his schoolfellows, some were remarkable for verbal memory, remembering words even which they did not understand; while others were deficient in this qualification, but recollected readily facts and events. Others were distinguished for the facility with which they remembered places; some would repeat a piece of music faultlessly, which they had heard but once or twice; while others principally recollected numbers, others dates, and so on. No individual, however, in Gall's experience, seemed to excel in *all* these talents in a like degree. These circumstances induced him, on discovering a special memory, to

regard it as a distinct power, in probable alliance with a particular part of the brain.

In society, he noticed persons who, though not always profound, were learned, had a superficial knowledge of the arts and sciences, and knew enough to be capable of speaking on them with facility: in such persons, he found the middle of the lower part of the forehead very much developed. At first, he regarded this as the organ of the *memory* of things; but, on farther reflection, he considered that such a designation does not include the whole sphere of activity of the power which he recognised. He observed, that persons who had this part of the brain, not only possessed a great memory for facts, but were distinguished for prompt conception in general, and an extreme facility of apprehension—a strong desire for information and instruction—the "appetite for knowledge." He therefore rejected the designation of *memory of things*, and adopted the appellations, *Sense of Things*, *Educability*, *Perfectibility*, to distinguish the capacity which he had observed in coincidence with the development before mentioned.

Dr Spurzheim, from observation and reasoning, arrived at the conclusion, that Gall's facts comprised manifestations of two separate faculties, whose cerebral organs were distinct—the one, *Individuality* (according to an improved nomenclature), being placed below the other, *Eventuality*. The function of the former, according to Spurzheim, is to render the mind observant of objects which exist, forming the class of ideas represented by substantive nouns, as *man*, *tree*, *animal*. Persons in whom it is large, will examine particular objects with pleasure, irrespective of any purpose to which they may be applied. The faculty prompts to observation, and is a great element in a genius for those sciences which consist in a knowledge of specific existences, such as natural history, mineralogy, conchology, and so on. No doubt can exist with respect to Gall's more general

observation, and very little, if any, as to the separate function of the inferior division of the cerebral portion appropriated by Gall to the "sense of things."

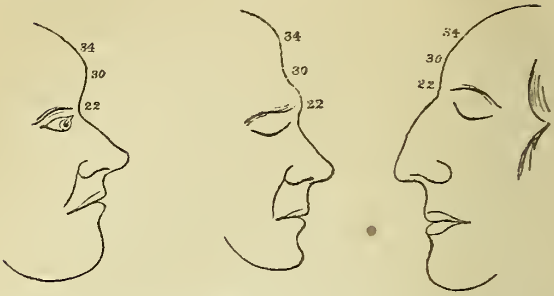
The office of *Eventuality* would appear to be to take cognisance of changes, events, or circumstances; differing from Individuality thus,—by the expression, the MAN *is*, a fact is stated which the latter faculty apprehends; when it is said, the MAN *dies*, an occurrence is affirmed which Eventuality cognises. Persons who delight in the relation of historical events, and who abound in what is called general information, have the forehead full in the region of Eventuality. Observation will very soon satisfy the unbiassed inquirer of these things. An anecdote is related by Mr Combe, in his System of Phrenology, which illustrates, singularly well, the outward conditions in immediate relation with the faculty now under discussion, and at the same time, admirably contrasts the speciality of this power with that of Individuality; the anecdote is as follows:—

"In visiting the State Prison of Connecticut, in the United States, on the 22d October 1839, I observed that the head of Mr Pillsbury, the superintendent of the prison, presented a deficient organ of Individuality, with a large organ of Eventuality; on which the Rev. Mr Gallaudet, who accompanied him, without giving any explanation to Mr P. of the subject of his inquiry, asked him whether, in seeing a review, he would observe, and recollect best, the appearance of the men or the evolutions; he replied instantly, 'the evolutions.'"[1]

The outlines given below exhibit, in the case of Pitt, a large Eventuality 30, and a moderate Individuality 22; Moore, the poet, presents a large Individuality and a small Eventuality; Sheridan's forehead displays both organs largely developed.

[1] System of Phrenology, vol. ii. p. 94.

PITT. MOORE. SHERIDAN.



In estimating the development of these organs on the living head, the breadth of the space at the root of the nose between the inner angles of the eyebrows should first be regarded; then the depth of this part of the forehead must be taken into account, which cannot be done unless the subject of examination be viewed in profile; this estimate may be formed by noticing the degree of projection anterior to the Sylvian fissure, the situation of which is laterally bounded by the inferior edge of the coronal suture, which corresponds pretty exactly with the most prominent portion of the malar bone.

In judging of Individuality, a difficulty will often arise owing to the presence to an undue extent of the frontal sinus; yet, however this may occasionally interfere with the *application* of Gall's discovery in this particular, it does not affect the validity of the discovery itself, even in the individual instance. In young persons in whom the sinus is not formed, the correspondence in power and development may be noticed; and the negative evidence at all ages supplies proof, in this way:—when the forehead at the root of the nose is narrow, contracted, and depressed, the subjacent brain is unavoidably small, and the particular power is *universally* weak; reasoning from this invariable sequence, the conviction may be gained, *philosophically*, of the reality even of this organ, a conviction,

moreover, not to be shaken by the difficulties which at times may arise from the presence of the frontal sinus.

Locality. Gall had always had a taste for natural history. This led him frequently to the woods to seek for birds' nests; he was accomplished enough in the pursuit; yet when he wished to return to the nests, he had always a difficulty in finding his way, and could rarely discover the particular tree that he sought, or the snares that he had laid. A school-fellow, named Scheidler, often accompanied him in these excursions. This young man, without any effort, always went directly to the place where a snare was set, although they had sometimes placed ten or fifteen in a quarter not familiarly known to them. In other respects, this youth possessed but very moderate abilities; a circumstance which caused Gall to ask, how it happened that he could hit his way so exactly on all occasions; the reply to which question was another interrogation, as to how it happened that Gall contrived to *lose* his way perpetually. In the hope of one day receiving more information upon this subject, Gall took a cast of his head.

Afterwards, he endeavoured to discover other persons distinguished in the same particular. The celebrated landscape painter, Schœnberger, related to him that, in his travels, he was in the habit of only making a very general sketch of the countries which interested him; and that, afterwards, when he wished to produce a more complete picture, each tree, each group of bushes, each stone of any material magnitude, presented itself spontaneously to his mind. A cast of the head of this painter was taken and placed by the side of that of his fellow student Scheidler. At this time, Gall made the acquaintance of M. Meyer, author of the romance of *Dia na Sore!* this person experienced no happiness but in a wandering life. Sometimes, he went from one country house to another; at others, he attached himself to some rich man as a travelling com-

panion. He had a remarkable facility of recalling to mind the different places he had seen. A cast of the head was again taken, which was placed by the side of the other two. The three heads were then compared with the greatest attention. They offered great differences in many respects, but that which struck the observer was, a singular conformation which all three possessed in the region immediately above the eyes, on the two sides of Individuality; this consisting of two distinct prominences which commenced near each side of the root of the nose, and which, going obliquely upwards and outwards, passed towards the middle of the forehead. From the time of making this observation, the idea arose in his mind, that the faculty of remembering places might be primitive, having its cerebral organ in the region just indicated. Subsequent observations confirmed this notion. Mr Combe, and most other phrenologists, regard the existence and function of this organ as established, although they do not altogether coincide as to the exact nature of the fundamental power; still, possession of the development in a high degree is very generally considered, conformably to the observations of Gall, to be necessary to facility in *place-finding*. Upon this matter, the author's own experience has been but inconclusive and unsatisfactory to himself; he cannot, from his own observations, feel that confidence in the case of *locality*, which he feels in so many other instances. A figure would not very well illustrate the appearance of this organ, either in excessive or in defective development.

FORM. Dr Gall remarked that certain persons, and even animals, recognise, with the greatest facility, individuals whom they have not seen for years; and even then, seen only in passing. In his own case, this faculty was very weak, and the defect was often a source of considerable annoyance to him. Amongst all classes of people, he noticed persons at the two extremes in this respect. Being desired to examine the head of a young girl who had an

extreme facility of distinguishing and recollecting persons, he found her eyes pushed laterally outwards, the intervening space being considerable. After many additional observations, he recognised an organ of a power for knowing persons, regarding the physiognomical sign just indicated, as the result of excess in development of a particular portion of brain in the inferior part of the anterior lobe. Dr Spurzheim, reasoning that persons became recognised by their *forms*, inferred that the primitive function was to take cognisance of configuration generally. The present writer has observed many cases corroborative of this organ.—It is generally regarded as established.

LANGUAGE. In the ninth year of Gall's age, his parents sent him to reside with one of his uncles, who was a curate in the Black Forest. This person associated another boy of the same age with Gall, in order to excite his emulation. He was often reproached, however, for not learning his lessons so well as his fellow-pupil, although more had been expected from him. After some time, both Gall and his associate went to a public school, containing about thirty scholars; here, whenever learning by heart was in question, he had to fear the very persons who, by their original compositions, could only obtain a seventh or even a tenth rank. Two scholars, in particular, surpassed even his ancient rival in the facility with which they learnt by heart. As both had very prominent eyes, they were nicknamed *ox-eyes*. Three years afterwards he changed his place of education; and, in his new school, was again doomed to meet with mortification in the affair of learning by heart, being constantly surpassed in this respect by *ox-eyed* scholars. Two years later, he went to study at Strasburg, and continued to remark that those students who very readily learnt by heart were distinguished by the same sign, although possessed of but very moderate abilities generally. Although Gall, during these observations, had no kind of preliminary knowledge fitted for guiding him to

the true interpretation of the phenomenon in question, he could not avoid the conclusion, that prominent eyes were an external mark of *good memory*. It was afterwards that he connected the sign with *verbal* memory simply, as other observations enlarged his knowledge and corrected his earlier inferences. Gall states it as a singular fact, that although these early observations were the first of the kind that led to his future researches, the exact conclusions which they afford always constituted a source of the greatest difficulty to him. In speaking of the physiognomical sign, Gall employs the French idiom, "Les yeux *à fleur de tête*," literally, Eyes on a level with the head. This appearance is produced by convolutions of brain, lying on the posterior and transverse part of the upper orbital plate, pressing the latter, and, with it, the eyes, forwards, downwards, and outwards, more or less, according to the development of cerebral structure.

Gall thought that there were two distinct faculties for language, and indications externally to correspond. The *prominence* of the eye he conceived to be produced by portions of brain, which, in excess, cause shallowness of the orbit, in consequence of their situation behind the posterior third of its external wall; and these he believed to be in subservience to mere knowledge of words. The depression of the eyes, and its anatomical cause, Gall refers to as follows:—"When the greater part of the middle portion of the inferior-anterior convolutions placed upon the supra-orbitar plate is very much developed, this part, the vault of the orbit, is not only flattened, but even depressed. In this case, the eyes are at once *à fleur de tête*, and depressed towards the cheeks; so that a certain interval is appreciable between the globe of the eye and the superciliary ridge. The eye, thus depressed, augments the slope of the inferior arch of the orbit." He then goes on,—"Persons who have the eyes in this way, not only possess an excellent verbal memory, but

they experience a particular vocation for the study of languages."[1]

Upon any details affecting this question, the author believes that very little positive knowledge exists. Observations and critical disquisitions without number have been made upon the organ, or organs, of language; beyond the fact, however, originally observed and recorded by Gall, that eyes prominent—*à fleur de tête*—indicate good verbal memory and facility of expression, the present writer doubts the existence of any demonstrative certainty. It is, however, no part of his design to enter minutely into any of these discussions; his present object being little more than to record the discovery of the individual organs, real or supposed; and this only in so far as it exhibits the *method* pursued. Any inquirer may satisfy himself of the reality of the fact in question, up to a certain point, by resorting to some public school, and requesting that a given number of boys be brought up, remarkable for talent in *getting off their tasks*, irrespective of any other capability,—and then, that a like number of scholars, equally remarkable for deficiency in the same particular, be grouped near the others; an inspection would be sure to show prominent eyes in the former class, and eyes sunken, comparatively, in the latter.

COLOUR. The internal appreciation of *colour*, as distinct from the faculties cognising other qualities of objects, is set forth by Gall and his disciples as depending upon that portion of the brain which is situated under the middle of the arch of the eyebrows. Gall records cases of greater or less development in this region, corresponding with the strength of the faculty of discriminating colours; and in the Edinburgh Phrenological Journal, and in the works of Mr Combe, much confirmatory evidence is adduced. In this instance, Gall does not relate the precise mode in which the idea first arose in his mind, nor what were the detailed

[1] Sur les Fonctions du Cerveau, vol. v. pp. 30, 31.

proceedings by which he established the conclusion to the satisfaction of his own mind. The present writer, either in consequence of natural inaptitude for minute observations on his own part, or from some other cause, has never felt any great degree of confidence in the observations recorded with respect to the smaller organs assumed to be situated beneath the superciliary ridge; although he thinks it a highly probable notion, that the internal faculties for cognising the general qualities, or *accidents*, of objects (individuality appreciating *substance*), have their cerebral connexions in this particular region; this seems probable from metaphysical considerations; and his own *general* observations have certainly strengthened the idea; but he confesses himself to be decidedly unsatisfied as to the particular organs. He apprehends that negative evidence, in the proof of such instances as that of colour, must be looked to for the strongest facts; if depression in some particular point of the arch of the eyebrows *always* coincide with defective appreciation of some quality in objects, colour for example, the conclusion he conceives to be warranted; but the bony inequalities of this part of the forehead must ever, he thinks, render valueless the positive facts for the purposes of proof, as an uncertainty must always exist whether a full development, or projection, be caused by bone or by cerebral substance. But whatever be the validity, or otherwise, of these particular details, the *method* of investigation pursued by the phrenologists is just and philosophical; and *that* is the subject with which the writer, at present, is more immediately interested.

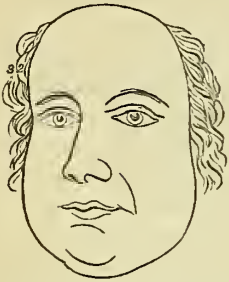
Tune. In the early stage of Gall's observations, when he was looking rather for outward signs of different kinds of memory, than for faculties in connexion with distinct cerebral organs in a more physiological sense, a girl, named Bianchi, scarcely five years old, was taken to him, and he was asked for what talent this child was remarkable. He discovered nothing in her head which, judging from

his past experience, was indicative of any particular memory; and the idea of an organ or outward sign of music had not yet occurred to him. He was told by his friends that little Bianchi had an extraordinary talent for remembering music; she would repeat whatever she heard sung, or played on the piano, and recollected entire concerts that she had heard but twice. Gall inquired if she learned by heart with equal facility, but was told that she was distinguished only in this particular. Hereupon, he thought that the talent for music was special; although, in the case of the little girl, he did not attach it to any conspicuous development. He prosecuted his researches with renewed ardour, examining the heads of musicians whenever he encountered them. Accidentally, he met several in whom the superior lateral part of the forehead was very narrow, and the temporal part very broad. As at this time he was but little advanced in his researches, not having begun systematically to assign special cerebral parts to special faculties of the mind, but looking rather for cranial *forms* indicative of particular talents, he first thought that a forehead displaying the figure of a truncated cone, like what he had observed in certain musicians, might constitute the external sign of a talent for music. Observing the heads of some distinguished persons in this department, including Beethoven, Mozart, and others, not to present this character of forehead, he was compelled to relinquish the notion. As Vienna furnished abundant examples of musical talent, he obtained casts from the heads of several persons remarkable in this way, in order to have an opportunity of comparing them with great care. At last, he succeeded in discovering a region, in which all the instances had a cranial prominence. He next observed all sorts of persons, children and adults, who had no kind of liking for music, some whom he examined expressed a positive disrelish for it; in these, he found the corresponding region decidedly flat and un-

developed; he procured skulls that had appertained to great musicians, and every thing confirmed the discovery.

The organ of *Tune* is situated at the middle of the lateral portions of the forehead, giving to this latter, when largely developed, a rounded appearance. Great practice is necessary for qualifying to observe this organ successfully in all cases; extreme instances, however, contrasted, will always lead to its verification. The two figures below may furnish some idea of the appearances induced by a large and by a small development (32).

HANDEL.



ANNE ORMEROD.

The one is an outline of Handel's forehead; the other is that of one Anne Ormerod, who was admitted, at twelve years of age, into the Blind Asylum at Liverpool. During two years, this latter was instructed in music, and no pains were spared in the attempt to cultivate and improve any talent in this respect which she might possess, but it was all in vain; her teachers were at last compelled to abandon the attempt. The organ is well established by numerous observations.

NUMBER. At Vienna, there was much talk of a scholar of St Pœlton who was distinguished throughout the country about, for his talent for numerical calculation. The boy was the son of a blacksmith, and had received no better

education than his ordinary associates, to whom he was very much the equal in general ability and acquirement. Dr Gall caused him to be brought to Vienna, and he was presented to his auditors; at this period the boy was nine years of age. He exhibited wonderful feats of mental calculation, accomplishing difficult problems in arithmetic more readily, and in less time, than the spectators with pen and paper in hand. The child, like other such prodigies, had created a method of his own, whereby his calculations were wrought. The circumstance excited considerable attention in Vienna; and a barrister of that city stated to Gall, the regret which he had that his own son, only five years of age, occupied himself almost exclusively with numbers and calculation, so that it was impossible to fix his attention on any other object, even the games of youth. Dr Gall compared the heads of the two children, and found no particular resemblance, except in a remarkable prominence at the external angle of the eye, and a little to the side. In both, the eye was in some measure covered by the eyebrow at its outer angle.

These two examples of distinguished arithmetical genius, coinciding in the conformation of a particular region of the head, suggested to Gall that the talent for calculation might be a fundamental faculty dependent upon a special cerebral organ; for, at this time, he states himself to have made considerable progress in his theory of the plurality of organs. In order to verify, or to explode, his notion, he sought out men distinguished for the power in question. He repaired to the Councillor Mantelli, whose favourite pursuit was to invent and to solve mathematical problems, especially in arithmetic, and he discovered in him the configuration before mentioned. He next went to Baron Vega, author of Tables of Logarithms, at that time Professor of Mathematics, and who, in every other talent, was only commonplace; the same peculiarity was observable, also, in his head. He then visited private families and schools,

and desired the children distinguished for ability in calculation to be pointed out to him; the same development always presented itself. "How could I do otherwise," says Gall, "than consider the sense of numbers to be a special faculty, and admit the existence of a particular organ for this faculty?"

A selection of extreme cases, positive and negative, will satisfy any unbiassed observer that Gall was right. The author's own observations leave no doubt upon his own mind that the organ is established.

Constructiveness. It will have been gathered from previous accounts, that, in the early stages of Gall's career, his object was to obtain physiognomical signs, in the cranial conformation, of particular talents; it was not until he had made some advances in this inquiry, that he interpreted the facts in the sense in which they are now generally regarded. It was very often the *general form* of the head which he sought, as an indication of aptitude for particular pursuits; afterwards, from increased knowledge and continued reflection, he attained the conclusion, that the subjacent brain was the special organ of the individual faculty, just as the different nerves subserve the respective manifestations. Thus he directed his attention to the cranial configuration of celebrated mechanicians, and was struck with the circumstance, that the heads of such artists were frequently as broad in the temporal region as at the cheek-bones. Yet although this occurred very often, it was not constant; nevertheless, he was more and more convinced that mechanical talent was special, and he continued his search for an outward sign in the head. He everywhere sought the acquaintance of distinguished mechanicians, studied their cranial configuration, and took casts of their heads. Whilst engaged in these researches, he met with two persons very celebrated for ability in this respect, and in these there appeared two swellings, round and distinct, at the temples; provision-

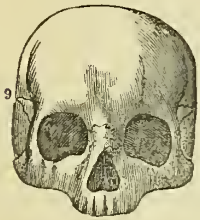
ally, he assumed this characteristic to be the external indication of mechanical talent.

Submitting the idea to the appropriate test, Dr Gall most assiduously multiplied his observations. At Vienna, some gentlemen of distinction brought to him a person concerning whose talents they solicited his opinion. Gall, from the development which he witnessed, stated that he ought to have a great tendency towards mechanics. The gentleman imagined that he was mistaken, but the subject of the experiment was greatly struck with the remark: he was the famous painter Unterbergen. To show that Gall had judged with perfect accuracy, he declared that he had always had a passion for the mechanical arts, and that he painted only for a livelihood. He took the parties to his house, and showed them there a multitude of machines and instruments, some of which he had invented, and others improved. Several of Gall's friends spoke to him of a man who was gifted with an extraordinary talent for mechanics; Gall described to them beforehand what form of head this person ought to have, and then they went to visit him. It was the ingenious mathematical instrument maker, Lindner of Vienna; and his temples rose out as in the former instance. Many years afterwards, at Paris, Prince Schwarzenberg, then the Austrian minister, wished to put Drs Gall and Spurzheim to the test. When they rose from table, he conducted Dr Gall into an adjoining apartment, and showed him a young man: without speaking a word, he and the prince rejoined the company, and Dr Spurzheim was requested to go and examine the young man's head. During his absence, Gall told an assembled company what he thought of the youth. Spurzheim very shortly returned, and said that he believed him to be a great mechanician, or an eminent artist in some branch demanding *constructive* talent. The prince, in fact, had brought him to Paris on account of his great talents in this respect, and supplied him with the means of prosecuting his studies.

The recorded observations of Spurzheim, Combe, and others, confirmatory of Gall's accuracy in this matter, leave very little doubt that some special property of the mind, concerned in mechanical talent, has its cerebral seat underneath the frontal bone, just above the spheno-temporal suture. Some differences of opinion exist as to the precise character of the fundamental faculty; this circumstance, however, does not affect the validity of Gall's discovery, in so far as it is an affair of *fact*.

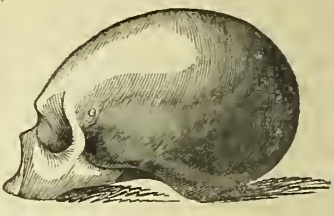
The skulls of the ancient Greek, and the New Hollander, seem to furnish contrasts in the developement of Constructiveness (9), corresponding with the recognised psychical aptitudes of the two races.

ANCIENT GREEK.



Large.

NEW HOLLANDER.



Small.

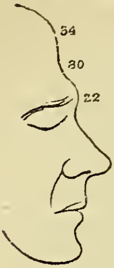
COMPARISON. Dr Gall frequently conversed on philosophical subjects with a *savant*, who was endowed with much vivacity of mind. Whenever the latter was embarrassed in any attempt to prove the accuracy of his views, he had recourse to a comparison. He would in this way strive to make good his argument; those with whom he debated, were often driven from their position and carried away by what he advanced, an effect which he could never produce by direct reasoning.

When Gall perceived this method of argument, he began to think that it might spring from some mental peculiarity, in

connexion with a particular form of the head. He had by this time learnt, from past experience, to look to the forehead for signs of special traits in intellectual manifestation; and in the instance just mentioned, his attention was soon engaged by a fulness in the upper and middle region of the forehead. The tendency to resort to analogy, or to comparative illustration, in reasoning, was at once associated in Gall's mind, with the cerebral structure producing the development noticed. Observations were accumulated, and they all corroborated the first idea. There were two ex-jesuits, in particular, whose heads came into the possession of Gall. These ecclesiastics, in their day, had been most successful preachers; they had attached to themselves both the educated and the unlearned; their sermons were chiefly remarkable for the profusion of parables, and the abundance of illustration by comparison. Further, there was the head of one Father Barhanmer, a renowned popular preacher: close reasoning did not characterise his discourses; but he swayed his auditors, and suspended their very breath, by his numberless comparisons, always taken from the ordinary affairs of life. In these three heads, the anterior, superior, and middle part of the forehead exhibited the same prominence as the subject of Gall's first observation. Subsequent experience, both on his own part and that of others, has rendered it pretty well established, that the disposition and power to reason by analogy, in opposition to a more direct process, have some dependence on the portion of brain in question, though it may be difficult to determine the precise character of the primitive faculty. Much speculation, and many differences of opinion, have prevailed amongst phrenologists upon this subject; these circumstances, however, leave the observed *facts* untouched.

The development is conspicuous in Moore, and only moderate in Sheridan, as illustrated by the subjoined outlines at the positions marked (34).

MOORE.



SHERIDAN.

CASUALITY. At Vienna, Dr Gall remarked that in the most zealous disciples of Kant,—men distinguished for profound, penetrating, metaphysical talent,—the parts of the brain lying immediately at the sides of the organ of Comparison were distinctly enlarged. He and Dr Spurzheim subsequently saw a mask of Kant himself, moulded after death, and observed an extraordinary projection of these parts. At a later period, they became personally acquainted with Fichte, and found in his head a still larger development of that region than in Kant's. Innumerable facts have satisfied all observers that the disposition and the power to reason directly from effect to cause have a cerebral connexion, as pointed out by Gall. In judging of this development, as also of Comparison, the method of examining in profile, as before indicated, must be practised. Extreme cases, positive and negative, may soon convince any one of the truth of phrenology in this particular.

WIT. Dr Gall states, that in the heads of Rabelais, Cervantes, Boileau, Racine, Swift, Sterne, and Voltaire, the antero-superior and lateral parts of the forehead are prominent and rounded; he considered that the *wit* (a term notoriously difficult of precise definition, though pretty well understood in its general sense) which characterises their compositions, flows from some mental peculiarity—some primitive faculty—located in the region just referred to.

In Dr Spurzheim's first English work, the faculty was discussed as an intellectual power; but, in his French and later English works, it is considered rather as a sentiment, and in this opinion Mr Combe and the Edinburgh school of phrenologists generally coincide. In the various essays which have been written upon this matter, speculation and ingenious dogmatism, in the judgment of the present writer, have been far more actively at work than rigorous induction. For his own part, although Mr Combe and many others have regarded this presumed organ and its essential functions as fairly made out, he has never been enabled to satisfy himself that *any* discovery has been made upon the subject; yet he has *observed*, anxiously and often. It has appeared to him that the things compared in the case of Wit have not always been the same; that one phrenologist has had one idea of the quality, and another, another; and that cases have dwelt upon the mind of the observer, only when the coincidence has been found. He has thought that Spurzheim generalised prematurely from Gall's few facts; and that the authority of the former has wedded phrenologists to the idea; and that the difficulty of defining the quality in terms, and the facility of explaining results, by combination of other faculties, when the development is absent, has tended to keep up the premature assumption. He may be wrong;—but he sincerely believes that the supposed region of the organ of wit, or mirthfulness, is *terra incognita*. The author's immediate business, however, is rather with the principle involved in the *method*, than with the *results*, of these investigations; and, in pursuance of this object—the enforcement of a just method of determining relations between structure and function,—he would suggest that some acute and powerful observer should tax himself with the discovery of the function of this cerebral part, in the following manner:—Let him begin by discarding for a while all metaphysical refinement based upon a few selected cases; let him collect records of the mental

peculiarity of *every* individual encountered, who is characterised by the development in question; when the accumulation has become considerable, let him examine the records in order to see what is common, or nearly so, to every fact in the series, striking out of the result every circumstance not, in some way or another, common to the whole; what is left may go far to suggest the special manifestations of the organ under scrutiny. Let the proceeding then be followed up by obtaining, *indiscriminately*, negative instances in large number; if it should be found that, in *all* cases of deficiency in organic size, a defect is appreciable in the peculiarity which distinguished the positive class, the *proof*, it is conceived, would be tolerably complete. All this would constitute the method of induction.

Ideality. This is a name which is given to the feeling that constitutes the essential soul of poetry. Gall spoke of the "poetic talent." The first poet whose head arrested his attention by its form, was one of his friends, who composed verses *extempore* when least expected to do so, and who, in consequence, had acquired some reputation, though but an ordinary person in other respects. His forehead, immediately above the nose, rose perpendicularly, then retreated, and extended itself a good deal laterally, as if a part had been added on each side. In other poets he did not find, as a constant occurrence, the forehead first perpendicular and then retreating, so that, in this respect, he regarded the shape of his friend's head as an accidental circumstance; but he states that in all poets he observed the prominences in the anterior-lateral parts of the head, above the temples. Hereupon he began to think that these prominences might indicate a natural talent for poetry; yet he spoke to his class with doubt. He hesitated, until continued and extended observations should confirm and establish his notion.

A short time afterwards, Gall procured the head of the poet Alxinger, in which this part of the brain was very

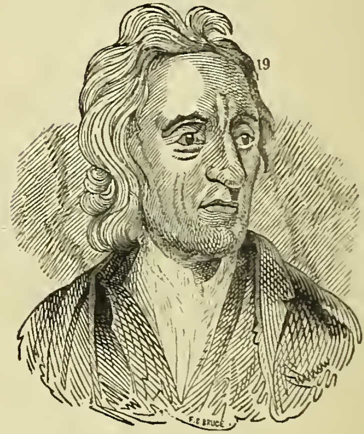
much developed. A little after this circumstance, the poet Junger died, and he found the prominences also in this head. He found the same parts still larger in the poet Blumauer. At this period Wilhelmina Maisch acquired reputation at Vienna by her poetry; and the same enlargement above the temples was found in her head. The same organisation was observed in Madame Laroch, at Offenbach, near Frankfort; in Angelique Kaufman; in Schiller, of whom he had a mask; and also in Gessner of Zurich. In Berlin, some years after his first observations, he still spoke of this organ with some reserve, when M. Nicolai invited him and Dr Spurzheim to see a collection of about thirty busts of poets in his possession. They found, in every one of them, the part in question projecting more or less considerably, according as the talent was manifested more or less in the individual poet.

In Paris, Dr Gall moulded the head of Legouvé after his death, and found this organ large. He and Dr Spurzheim opened the head of Delille, and pointed out to several physicians who were present, the full development of the convolutions placed under the external prominences at this part: these convolutions projected beyond all the vicinant ones. In a pretty numerous assemblage, Dr Gall was asked what he thought of a little man who sat at a considerable distance from him. As it was rather dark, he replied that, in truth, he could not see him very distinctly, but that, nevertheless, he observed the organ of poetry to be greatly developed. He was then informed that this was the famous poet François, generally named Cordonnier, from his having been brought up as a shoemaker. From these, among other circumstances, Gall finally taught, that the talent for poetry depends upon a primitive faculty, and that it is connected with the cerebral region before indicated.

The following figures represent the organ large in Chaucer, and small in Locke (19).

CHAUCER.

LOCKE.



Although very little doubt can exist with respect to the substantial reality of this discovery, the author is not satisfied that any very exact notion of what the primitive quality is, has been made out. His facts and reasons are

numerous, but his object being, not to settle disputed points in the science, but only to show the method by which they may be determined, he does not consider it proper here to enter into details.

BENEVOLENCE. One of Gall's friends frequently said to him: "As you are engaged in seeking external signs which indicate internal faculties and qualities, you must examine the head of my servant Joseph. It is impossible to find goodness superior to that of this young man. For upwards of ten years that he has been in my service, I have never seen any display but that of kindness and goodness of heart, —a surprising thing in a man who, devoid of education, has been brought up among servants of very inferior habits." Although at this time Gall had no idea of locating *goodness of heart* in the brain, nor consequently of seeking for an outward sign of that quality in the cranial configuration, the reiterated remarks of his friend awakened his curiosity.

He called to mind the habitual conduct of a young man whom he had known from infancy, and who was distinguished from his numerous brothers and sisters by his excellence of disposition. Although the youth was passionately fond of the games proper to his age, and delighted to range the forest in search of birds' nests, yet no sooner did any of his brothers or sisters fall sick, than an inclination still more powerful kept him at home, and drew from him the most assiduous attentions towards the sufferer. When grapes, or apples, or cherries, were distributed among the children, his own share was always the least, yet he rejoiced in seeing the others partake more largely than himself. He was never more pleased than when some good fortune happened to those whom he loved, on which occasions he would often shed tears of joy. He was fond of taking charge of sheep, dogs, rabbits, pigeons, and birds; and if one died, he wept bitterly, at which circumstance his associates would jest. These dispositions certainly did not arise from education. On the contrary, he had been all

along surrounded by those whose conduct was calculated to produce the very opposite results.

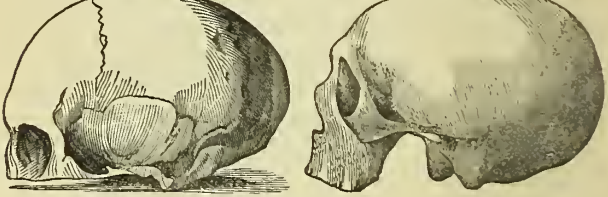
On one occasion, Gall was speaking, amidst a numerous family, of the boasted excellence of the servant Joseph. "Ah!" said the eldest daughter, "our brother Charles is exactly the same; you must really examine his head. I could not tell you how good a child he is."

"I had then before me," says Gall, "three cases, in which goodness of heart was strongly marked. I took casts of the three heads; placed them beside each other, and continued to examine them, until I should discover a trait of character common to the three. This I at length found, although the heads were, in other respects, very differently formed. In the mean time, I tried to find similar cases in families, schools, &c., that I might be in a condition to multiply and correct my observations."

The development discovered by Gall, in the above cases, consisted in a raised, arched appearance, at the anterior summit of the frontal bone, just above the organ of Comparison. The following cuts afford specimens of contrast in this particular (13); the skull of Burns being elevated high above the eyes, while that of Griffiths (a murderer), is low and flat.

BURNS. GRIFFITHS.

13 13



Every observer has witnessed instances corroborative of Gall's accuracy in this matter. The author inclines to say, with the discoverer, "that there is scarcely any fundamental quality or faculty whose existence and organ are better

established than that of Benevolence." It is, moreover, a point in phrenology, singularly easy of verification, for obvious reasons—the quality is readily appreciable, and the cranial form, when the organ is in excess, may at once be identified.

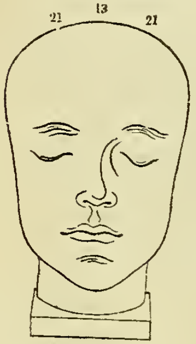
IMITATION. One day, a friend, with whom Gall was in conversation concerning forms of head, assured him that his own had a configuration quite peculiar. " He directed my hand," says Gall, " to the superior anterior region of the head. I found this region elevated in the form of a segment of a sphere, and behind the protuberance there was a transverse depression in the middle of his head." Hereunto, Gall had not observed this conformation. The person had a remarkable talent for mimicry, imitating the walk, the gesture, the sound of the voice, in a manner so striking that the person imitated was at once ascertained. He repaired immediately to the institution for the deaf and dumb, in order to examine the head of a pupil named Casteigner, who only six weeks before had been received into the establishment, and from his entrance had attracted notice by his amazing talent for mimicry. One Shrove Tuesday, when a little play was performed at the institution, he imitated so perfectly the gestures, gait, and looks of the director, inspector, physician, and surgeon of the establishment, and above all of some women, that it was impossible to mistake them. This exhibition was the more amusing, as nothing of the kind had been expected from the boy. To Gall's great astonishment, he found the same part of the head elevated, as in the case of his before mentioned friend.

" Is the talent for mimicry, then," said Gall, " founded on a particular faculty and organ?" He sought every opportunity of repeating his observations. He visited families, schools, &c., examining the heads of all individuals distinguished for their powers of mimicry. At this time, Mr Marx, secretary to the minister of war, had acquired a

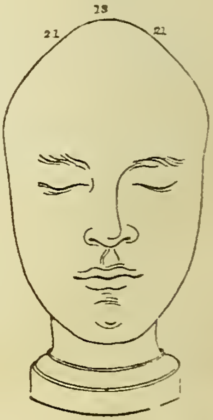
great reputation by playing several characters in a private theatre. The same development as that which had been noticed in the two first cases, was discovered here, and in all succeeding cases; generally, to an extent proportionate to their possession of the corresponding talent. The heads of actors were examined, and these constantly presented the same characteristics when the power was great. He procured the skull of Junger, a poet and comedian, and afterwards used it to demonstrate this organ. Subsequently, he and Spurzheim, in their travels, found the same organisation in all the great actors whom they had an opportunity of examining; many of the leading ones Gall enumerates by name.

The observations of Mr Combe, and others, in confirmation, leave very little doubt upon this organ; it seems to be established. Its peripheral expansion is longitudinally backwards above, and somewhat external to, Causality, on each side of the organ of Benevolence. Some idea of the different appearances which the development presents, when large and small, may be formed by studying the subjoined outlines. One figure represents Miss Clara Fisher, who, at eight years of age, exhibited great talents

MISS C. FISHER.



JERVIS.



as an actress; the other one, Jacob Jervis, is remarkable for benevolence.

In both these figures, the head rises high in the region of Benevolence; but in Jervis there is a rapid slope on the two sides, indicating a deficiency in Imitation 21; whereas, in Miss Clara Fisher, it is as high at 21, showing considerable development in both organs.

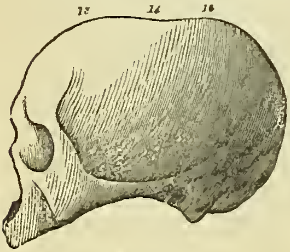
Veneration. Gall states that he was one of ten children; all, in childhood, received very much the same education, but their talents and dispositions were very different. One of his brothers had an extremely pious nature, manifested from the tenderest infancy. His very recreations bore a religious character. He was constantly occupied in religious exercises, hearing mass every day. When compelled to be absent from church, he spent his time at home in beautifying a wooden crucifix. His father had destined him for commerce, but he entertained a decided aversion to the occupation of a merchant, because, said he, it exposes to the danger of lying. At the age of twenty-three he abandoned merchandise; and having lost all hope of then being enabled to pursue ecclesiastical studies, he fled from his father's house and became a hermit. The father at length, at Gall's solicitation, permitted this young man to study for the church. Five years afterwards he entered the priesthood, and lived till his death in habits of devotion and penitential exercises.

Gall further noticed, that in schools some of the children took no interest in religious instruction, whilst others received it with avidity; also that those individuals in the classes who voluntarily devoted themselves to the church, exhibited the inclination frequently without any intellectual qualifications for the vocation. This commendable feeling, says Gall, sprang up in them nobody knew how; and, in many cases, it certainly was not attributable either to example, education, or to the general circumstances in which they had been placed.

At a later period, when his attention was constantly fixed upon innate qualities of the mind, and their essential differences, he recollected these observations of his youth, and began to examine the heads of persons distinguished by devotional feeling, satisfied that it was innate. He visited the churches of all denominations, and particularly noticed the heads of individuals who prayed with the greatest fervour, or who were the most completely absorbed in religious contemplation. He soon observed that the heads of such persons were elevated in the centre of the coronal region. Being struck with this fact, he repaired to the convents, and noticed the monks. His observations were confirmed generally in the case of such as exercised the more sacred duties, as of preacher or of confessor; but not always in the instances of those occupying the inferior posts. The more his experience upon this point extended, the more certain did he feel that a special faculty is the source of devotional feeling, and that its cerebral seat is in the superior and middle region of the head. Numerous other observations, and the constant experience of all phrenologists, leave the truth of this fact beyond all dispute.

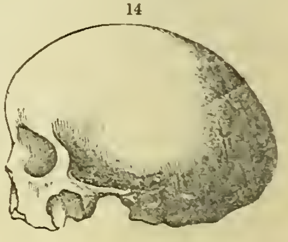
The configuration of the skull when the organ is large, and when it is small, is shown in the figures below (14).

DR HETTE.



Small.

GIRL.



Large.

The figure annexed is the head of St John the Evangelist, in which the high development of Veneration is

exhibited. It is from the portrait of the Last Supper, by Leonardo da Vinci.

ST JOHN.

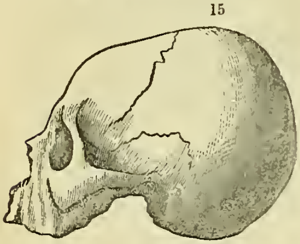
FIRMNESS. Gall observed that the posterior part of the coronal region, close upon the middle line behind Veneration is largely developed in persons of a firm and constant character, and thence concluded that the cerebral parts there located manifest the mental property which mainly contributes to fortitude, perseverance, and determination, and, in its abuse, to obstinacy and stubbornness. The quality will ever be recognised in the man who is "*tenax propositi*," corresponding with altitude in the proper region of the head; as also in children who are stubborn and intractable. Its deficiency will be noticed in persons who, in the prosecution of any object, are vacillating and un-

steady in the means which they employ; captivated by one project to-day, by another to-morrow, and by a third on the next day.

The cranial configuration in question was noticed by Lavater, as a mere physiognomical indication of the peculiar trait of character with which Gall's observations connected it. And, indeed, no fact in phrenology seems to be better established than the present one; no organ, in its high or low development, is more readily discriminated.

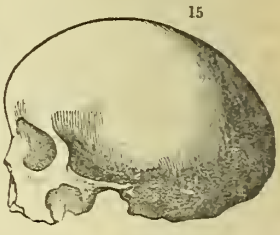
The woodcuts below represent appropriate cases for early observers. The region of Firmness is indicated by fig. 15.

FRENCH SOLDIER.



Large.

GIRL.



Small.

Conscientiousness. Most moral philosophers admit the existence of *conscience* in the psychical constitution of man, as a species of inner instinct which gives the love of the true in contradistinction to the false, of the real in opposition to the pretended, and of the genuine in contradiction to that which is factitious, producing the feeling of duty, obligation, and incumbency. Gall did not admit a separate faculty and organ of conscientiousness; but, towards the close of his career, regarded the sentiment as comprised in benevolence. Spurzheim, however, was satisfied, both from observation and reasoning, that the sense

of right and wrong was a distinct faculty, bearing no fixed relation to any other quality of the mind. Kind-hearted good-natured people were noticed, whose moral integrity was none of the strictest; and upright, rigorously just individuals were encountered, of no great benevolence of disposition. From these circumstances, he was led to seek for a distinct cerebral organ, which he ascertained to exist in the posterior and lateral parts of the coronal region, on each side the organ of Firmness. Dr Spurzheim does not detail the particular facts which guided him originally in the discovery, as was the custom of his great master. Mr Combe, however, in his System of Phrenology, treating of the organ of Conscientiousness, supplies some striking cases in illustration and corroboration of it, some of which may not inaptly be here epitomised.

In 1816, Mr Combe, having then but a slight acquaintance with phrenology, which he had gained from attending Dr Spurzheim's lectures, was requested to find out and engage a person accustomed to business, of strict integrity, to act as confidential clerk and cashier to a manufacturing company. An individual was recommended to Mr Combe, for the purpose, by an Edinburgh merchant, whom he had long known and esteemed: the qualifications as to integrity and general excellence being represented as of the highest order, Mr Combe's friends at once engaged him.

From the first, Mr Combe had been struck with a very moderate development of Conscientiousness in the head of this person; he had observed, also, that Firmness was not large. Having at that time, however, very little experience in estimating development, and no great reliance on phrenology as a science for practical application, he was not much influenced by the circumstance. The intellectual organs were large, as were those also of Benevolence and Veneration; Love of Approbation was very large. Mr Combe, nevertheless, expressed to the gentleman who had recommended him, his regret that the organs of

Conscientiousness were so palpably defective. To this, the reply on the part of the gentleman was, that he knew nothing about the effect of organs, but that he knew, from experience, the party recommended to be an honest man. The matter dropped; and for several years the person's conduct appeared to be irreproachable.

He was a leading member of a dissenting congregation, and his house was the resort of numerous friends for prayer. Mr Combe considered that there was compensation for a great natural defect in other principles. Time, however, evolved different views of his character. His cash-book became confused; the days for balancing it were postponed by him under a variety of pretences; the usual returns from the sales began seriously to diminish; and Mr Combe was led to insist on an investigation of his conduct and transactions. The result was an exposure of habits of peculation and embezzlement that had run over a long period. He confessed freely the use to which he had applied the fraudulently obtained money; he was not in the least addicted to sensual debauchery in any form; but his natural kindness of disposition, in alliance with vanity, had led to habits of extravagance and improvidence—moral principle not having been powerful enough to hold them in due check.

This case made a strong impression upon Mr Combe. It was conceived that, with the natural qualities of the delinquent, he ought not to have been exposed to the temptation involved in a situation of pecuniary trust; and, in this view of the case, he was dismissed from his employment, without prosecution. Subsequently, he entered into trade on his own account; and, after one or two bankruptcies, he left Edinburgh for the United States, where he was at times heard of as still living by the practice of plausibility and falsehood.

From this time forward, Mr Combe adopted the rule of engaging no one for his own service, without previous

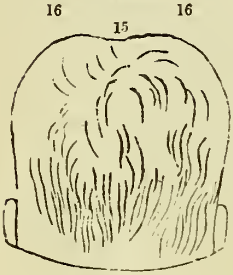
examination of the head. At the period of forming this regulation, however, he had already certain individuals in his employment, in whom Conscientiousness was deficient; these, from feelings of kindness, he did not dismiss. There was one who exhibited a large development of the intellectual organs, with large Benevolence, large Self-Esteem and Acquisitiveness, but with very deficient Conscientiousness: by this one, notwithstanding a vigilant superintendence which was exercised over his conduct, he was repeatedly cheated and plundered of sums of money. On one occasion, Mr Combe wished to hire a lad as a stable boy: one of thirteen or fourteen years of age was highly recommended by a woman from the country, whom he had long known and respected. She said that the boy was the son of one of her neighbours, that she had known him from infancy, and that Mr Combe might rely alike on his moral qualities and his knowledge of horses. On examining the lad's head, the intellectual organs, and also those of Benevolence, Veneration, and Firmness, were found to be well developed; but Secretiveness and Acquisitiveness were large, and Conscientiousness was very deficient. The woman who had recommended the boy was informed that he would not suit Mr Combe. She left the house in high indignation, that he should "reject honest men's sons from his service, because their heads did not please." In about a month she returned, stating, with some shame, that she had since learned he had been dismissed from his last place on account of thieving; and that in the locality where he had served, his character had become pretty notorious in that respect. A man of education and talent, relates Mr Combe, holding civil and ecclesiastical offices in Edinburgh, was noted for the active interest which he took in missionary societies and other such schemes. The cerebral development, on the whole, was not unfavourable, but there was great deficiency in Conscientiousness. After nearly thirty years of ostensible piety and respectability,

it was discovered that he had all along been deviating widely from the paths of rectitude: dishonesty and licentiousness were the conspicuous features of his delinquency. Another individual, with deficiency in Conscientiousness, was obliging and generous to excess, but reckless of justice: he borrowed money from all who would lend it to him, pledged his faith most positively to each lender to repay the loan on a particular day, but never thought more of the promise, until he was importuned or compelled to perform.

These cases, detailed by Mr Combe as examples, among others, occurring within his own experience, both illustrate and confirm the discovery of Spurzheim; and they are but specimens of what any one may realise by personal observation. Indeed, judging from this, irrespective of recorded cases, the function of the part in question is, to the satisfaction of the author's mind, perfectly established; he should almost as soon doubt the office of the optic nerve.

The appearance which the head presents in extreme cases, may be understood to some extent from the following cuts, which represent the head viewed posteriorly: fig. 16 indicates the locality of the organ.

MRS H.



Large.

DAVID HAGGART.

Small.

HOPE. Dr Gall considered this sentiment to be a *mode of action*, not a *primitive quality*, believing that hope appertains to every faculty. Spurzheim, however, conceived that, although every faculty may *desire*, "hope" is really a sentiment proper. Little doubt can exist upon this point, even were there no discovery, real or supposed, of a correspondent organisation. Numerous observations have rendered it in the highest degree probable, that the situation of the organ of Hope is on each side that of Veneration. It was Spurzheim who first suggested this locality, and many observations have confirmed the idea.

WONDER. Dr Gall made certain observations relative to the probable function of that part of the brain which is situated beneath the superior and lateral parts of the frontal bone, though he did not set forth any positive determination upon the subject. He noticed that some individuals are very prone to believe in ghosts and apparitions, and that this disposition occurs among the well-educated and instructed classes, as well as amongst the ignorant. Dr Jung Stelling, whom he often saw with the late Grand Duke of Baden, was a tailor in his youth, then a tutor, afterwards physician, moralist, journalist, illuminatus, and visionary. In him, this part of the brain was largely developed. He firmly believed in apparitions, and wrote a book in exposition of the doctrine. A gentleman who moved in the best society in Paris, asked Dr Gall to examine his head. The doctor's first remark was, "You sometimes see visions and believe in ghosts." The gentleman started from his chair in astonishment, and said that it *was* so, but that heretofore he had maintained silence on the subject, lest he should be ridiculed for his credulity. On another occasion, Gall told one Dr W. that he ought to have a strong liking for the marvellous and supernatural. He was informed that for once, at any rate, he was quite mistaken; that it was a rule with him, Dr W., to believe nothing that was not mathematically demonstrable.

Gradually, and by design, Gall advanced to the subject of animal magnetism, which appeared a fit topic for putting the mathematical rigour of his proofs to the test. He instantly brightened up; assured Dr Gall that in magnetism a spiritual being operated, even at great distances; that no extent of space obstructed its influence; and that on this account it could sympathise with persons in any part of the world. "It is the same thing," he went on, "that produces apparitions. Ghosts and visions are rare, no doubt, but they certainly exist; and I am acquainted with the laws which regulate their production." After this, Gall thought that he had not been so very far wrong. Several other such instances are recorded by Gall, whose inferences, however, did not extend beyond the following suggestions:—"Does this convolution form part of the organ of Imitation, and does its extreme development exalt the talent for mimicry to such a degree, as to personify simple ideas, and to give them, thus metamorphosed, a locality out of the individual? Or does it constitute parts both of Ideality and Imitation? Or, finally, does it constitute a separate organ? These points can only be determined by further researches." The facts and contingent conclusions just recounted admirably exemplify the cautious and truly philosophical spirit introduced by Gall into all his investigations.

After his separation from Gall, Spurzheim multiplied observations, and constantly taught the existence of an organ of *marvellousness*, as a branch of what was positively established in the particular physiology of the brain.

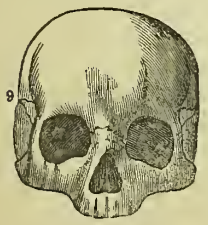
Mr Combe's facts and reasonings upon this point are thus summed up in the System of Phrenology:*—"I have met with persons excessively fond of news, which, if extravagant, were the more acceptable; prone to the expression of surprise and astonishment in ordinary discourse;

[1] 5th Edit., Vol. i. p. 455.

deeply affected by tales of wonder; delighting in the Arabian Nights' Entertainments, and the mysterious incidents abounding in the Waverley Novels; and in them I have uniformly found the part of the brain in question largely developed. When the organ predominates, there is a peculiar look of wonder, and an unconscious turning up of the exterior portions of the eyelashes, expressive of surprise. In other persons, I have found the part of the brain in question small, and in them it was accompanied with a staid soberness of feeling, diametrically opposite to the manifestations above described. Such individuals were annoyed by every thing new or strange; they scarcely felt or expressed surprise, and had no taste for narratives leaving the beaten track of probability or reality, and soaring into the regions of supernatural fiction. On analysing these manifestations, they all appear to be referable to the sentiment of wonder; an emotion which is quite distinguishable from those hitherto enumerated."

It is presumed that the organ, as associated with the disposition in question, is established; the writer's own observations would not warrant him in raising it above the *probable* grade. The developement is seen large in the subjoined figure of the ancient Greek skull, and small in the two Esquimaux skulls.

ANCIENT GREEK.



ESQUIMAUX.

In the Greek head, the full rounded aspect of the superior lateral regions of the frontal bone is very obvious; whilst in the Esquimaux, the low, narrow, and depressed character of the skull in the same part is equally obvious.

SIZE. Dr Spurzheim inferred by reasoning, that there must be a faculty whose function is to appreciate *dimension;* observation led him to conceive that he had discovered the organ for its manifestation, placed on the two sides of the lower portion of Individuality, and indicated externally, when large, by prominence at the internal extremity of the arch of the eyebrow. Mr Combe states, that the soundness of this conclusion is proved by experience. The author has no opinions of his own upon the subject; the remarks in reference to his own notions on these minor organs, made in discussing the organ of Colour, may in all respects apply here.

WEIGHT. Dr Spurzheim, judging that the faculty of estimating weight and resistance is included in no other, conceived that a separate organ for this power exists, situated between those of Size and Colour. Much ingenious speculation has been expended upon this supposed faculty, and various views have been advanced as to its sphere of action; to an extent, indeed, that has destroyed all confidence, on the part of the writer, in the validity of most of the observations that have been recorded on this matter. An ingenious theory has been constructed, and facts have readily been imagined to exist, and to present themselves corroborative thereof. The author can neither confirm nor controvert the reality of this organ of weight. Mr Combe states, that "persons who excel at archery and quoits, and also those who find great facility in judging of momentum and resistance in mechanics, are observed to possess this organ largely developed."

TIME. Phrenologists generally conceive that there is an organ for estimating *duration*, situated in the middle of the forehead, on each side of Eventuality, a little above, and

external to, Locality. "The power of conceiving time," says Mr Combe, "and of remembering circumstances connected by no link but the relation in which they stand to each other in chronology, and also the power of observing time in performing music, is very different in different individuals. The faculty gives the power of judging of time, and of intervals in general." All the reasoning is in favour of the existence of such a *faculty*. The writer would hardly say as much in favour of the facts cited in support of the *organ*, although he would rank it as highly probable.

ORDER. A sense of order in the physical arrangements of objects, is considered to depend upon a primitive faculty whose cerebral organ, largely developed, is indicated by a great fulness, that produces a square appearance at the external angles of the superciliary ridge. Gall states that he had met with facts corroborative of the notion that the sense of order depends upon a primitive faculty, observing at the same time, that on account of the difficulty of observing the organs placed about the superciliary ridge, and the small size of this alleged one in particular, he was not able to collect a sufficiency of determinate facts to authorise him to decide on its situation, as pointed out by Spurzheim. Mr Combe regards it as established.

CONCENTRATIVENESS. The region of the head below Self-Esteem and above Philoprogenitiveness, is a disputed territory, as regards its functions. Dr Gall had noticed a disposition to fix in high places, in certain of the lower animals, whose crania he conceived to exhibit considerable development in or about the situation just mentioned. This circumstance, however, was considered by him to exemplify a peculiar manifestation of Self-Esteem in these creatures. No determinate inference was drawn from such observations. Gall simply recorded the fact.

Dr Spurzheim, however, believed himself to have made out the functions of the part in question, in the recognition

of its excessive development in the heads of those whose attachment to particular places is remarkable. To judge from his language, it would seem that he was mainly guided to the notion by facts witnessed in the animal kingdom. In his work entitled *Phrenology* he observes, "I consider, in animals, the cerebral part immediately above the organ of Philoprogenitiveness, as the organ of the instinct that prompts them to select a particular dwelling, and call it the organ of Inhabitiveness. It is known that cats are more attached to places, and dogs to persons. The former remain in the house which is sold, while the latter follow their master." He states that his attention had been, and was still directed to such individuals of the human species as exhibited great regard for their dwelling-places. "I have many facts," says he, "in confirmation. I saw a clergyman in Manchester, known to his friends as particularly attached to his dwelling-place, so that he should be unhappy if obliged to sleep elsewhere. I examined his head, in company of several gentlemen, some of whom were opponents, but every one was obliged to admit that the spot of the head where No. 3 (Inhabitiveness) is situated, was warmer than the rest of the head." Some other *general* facts are stated by Spurzheim, but his conclusion has not, for the most part, been deemed valid. For several reasons, the author attaches but little value to what he has advanced, either in the way of fact or inference. Any objections which the writer may entertain on metaphysical grounds, it would be foreign to his present purpose to adduce. He must, however, take serious exception to the whole *method* apparently pursued by Spurzheim, in this particular instance, deviating so widely, as it does, from the rigorous and successful procedure of Gall,—constituting an example, among several, of his undue disposition to speculate, and to generalise prematurely, after his separation from the great discoverer. Our judgment concerning Spurzheim's proceedings, must be formed by study of his writings, and these

most decidedly indicate that the basis upon which he rested for proof of this organ of Inhabitiveness, was in natural history and comparative anatomy; and that he sought, from the human kind, facts in corroboration, rather than as direct evidence — a mode of procedure essentially wrong, for reasons amply stated in foregoing chapters. If an idea once fix itself in the mind, no matter in what way—let it be on grounds ever so unsure—such cases as that of the clergyman at Manchester may be found in abundance to confirm it. In this instance, the head, in the region of Inhabitiveness, was obviously *warmer* than the remaining surface. Yet how many sources of fallacy lurk in the determination of such a circumstance! May it not have been that *contact* was longer maintained in the region in question, in the expectation of realising the asserted fact? It is certain that if you apply the hand but slightly to one part, and intently to some other, the subjective impression of heat, in the absence of all difference in the object, will be greater in the latter case. But the author has been informed by a gentleman who was present on the occasion, that an increase in the local temperature was *not* so obvious; that to himself, indeed, it was not appreciable; and this gentleman is no opponent, but probably the earliest of Spurzheim's adherents in Manchester, and a personal friend. Further, the author has learned from the same authority, that the intensely *inhabitive* clergyman, who "should be unhappy if obliged to sleep elsewhere" than at his then home, did not decline subsequently a better living, although it involved a change of residence. Let us take this fact, moreover, in another aspect. The recorded excitement of the feeling was plainly of a *chronic* nature; yet it is only with temporary and acute excitement that analogy would justify us in associating probable elevation of temperature in the scalp. A student, plunged in profound and intense abstraction, may not unreasonably be expected to present a heated forehead; but no evidence exists to show that intellectual foreheads, *in-*

ordinary, are unduly warm. The other circumstances cited by Spurzheim are too vague to admit of any close criticism.

It is no wonder that a proposition so loosely advanced, and resting upon evidence so defective, has never been admitted by phrenologists as established doctrine. Respect for its propounder, rather than the constant testimony of certain facts, has kept up a degree of attention to it, which a conclusion arrived at by so vicious a process would not, under common circumstances, have received. The application of Gall's system of investigation to Spurzheim's view has not supplied any decisive corroboration of it, which indeed was hardly to have been expected; for when the way is missed at the outset, a sure goal is not very readily attained.

Mr Combe has for some years taught, that the particular division of the brain now under discussion is associated with the faculty of mental concentration; and he denominates the presumed organ that of *Concentrativeness*. It is difficult, by any brief definition, to render the idea to be attached to this term any plainer than it already is by employment of the term itself. In detailed explanation, however, Mr Combe describes it as an ability to detain the feelings and ideas in the mind so as to give them the quality of continuity; and the absence of it is evinced, he states, in persons who "experience great difficulty in detaining their emotions and ideas so as to examine and compare them." The following passage from the System of Phrenology will exhibit the facts and the reasonings by which Mr Combe supports his views.

"In conversing with some individuals, we find them fall naturally into a connected train of thinking; either dwelling on a subject which interests them, till they have placed it clearly before the mind, or passing naturally and gracefully to a connected topic. Such persons uniformly have this organ large. We meet with others who, in similar

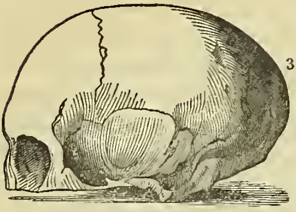
circumstances, never pursue one idea for two consecutive seconds, but shift from topic to topic without regard to natural connexion, and leave no distinct impression on the mind of the listener. This happens even with individuals in whom reflection is not deficient; but the organ in question is in such persons uniformly small. I have met a military officer, with Locality and Concentrativeness both large, who declared that he liked the stirring and diffuse life of a soldier, while engaged in active operations; but that when the army halted, he was equally pleased, and found equal facility, in concentrating his mind to reading, writing, or business, and was not annoyed by that dissipation of intellect of which many of his brother officers complained. On the other hand, a gentleman bred to the profession of the law, who has this organ rather deficient, declares that the effort of concentrated thinking is to him painful, though he has excellent Comparison, Causality, and Language."[1]

The author experiences the greatest difficulty in forming an independent judgment with respect to this alleged organ and faculty of Concentrativeness. When he has been disposed to test the proposition by his own opportunities of observation, he has generally had occasion to distrust the correctness of his appreciation of the mental quality. On the whole, he considers that, at best, this proposition is but rationally *conjectural;* and Mr Combe, he believes, does not rank it as *established.* But whatever may be thought of the precise character of the *results* of Mr Combe's investigations in this particular, the *method* assuredly is unexceptionable.

The annexed figures represent the development, large and small; the distance from the external opening of the ear to the situation of the presumed organ, 3, being in Burns considerable, and in the other comparatively slight.

[1] 5th Edit. Vol. i., pp. 214, 215.

BURNS.



Large.

NORTH AMERICAN INDIAN.

Small.

Alimentiveness. From several considerations, it early occurred to phrenologists, including Gall and Spurzheim, that the appetite for food was a mental instinct not referable to mere sensation, nor to any of the recognised qualities of the mind, but that it was primitive, and, in consequence, associated with a distinct cerebral organ. Mr Combe, Dr Hoppe of Copenhagen, and Dr Crook of London, all about the same time, appear to have pitched upon the convolutions anterior to Destructiveness as the organ in question, indicated to be in excess when the corresponding part of the head is full and prominent, causing great breadth generally in the zygomatic arch. Many observations have been made directly on man, and other confirmatory facts have been obtained from the lower animals which render it in the highest degree *probable* that the instinct for aliment is organically dependent upon the structure mentioned. Mr Combe regards this point as sufficiently well established by evidence to be considered certain. The writer has seen cases tending to the same conclusion, and none of a contrary character, but not in such numbers, nor of character so unequivocal, as to warrant any decisive speaking on his own part.

Love of Life. The varying degree in which individuals display this feeling, has rendered it probable that it is connected with some special part of the brain; and this by

analogy has been supposed to be at the base. Dr A. Combe has recorded the only fact, in detail, suggestive of its situation, in a case published in the third volume of the Edinburgh Phrenological Journal. In describing the dissection of the brain of a lady, upwards of sixty, who for many years had been remarkable for perpetual anxiety about her own death, Dr Combe observes as follows:—

"The enormous development of one convolution at the base of the middle lobe of the brain, the function of which is unknown, was too striking not to arrest our attention. It was that lying towards the mesial line, on the basilar and inner side of the middle lobe, and consequently of Destructiveness. The corresponding part of the skull showed a very deep and distinctly moulded cavity or bed running longitudinally, with high and prominent sides, and presenting altogether an appearance much more striking than in any skull I ever saw. Whether it may have any connexion with the Love of Life, is a circumstance which may be determined by future observations. All that we can say at present is, that the Love of Life seems to be a feeling *sui generis*, and not proportioned to any faculty, or combination of faculties, yet known; that, in the subject of this notice, it was one of the most permanently active which she possessed; and that, in her, the convolution alluded to was of very unusual magnitude; but how far the coincidence was fortuitous, we leave to time and observation to determine."

Dr Vimont, by observation on the lower animals, as well as on man, conceives that he has reason for coming to a more positive, though identical, conclusion respecting the function of the part in question; but the writer believes that Dr Combe's philosophically cautious statements embrace all that can be very well said upon the subject in the existing state of our knowledge.

Dr Vimont ranks amongst the most distinguished of the disciples of Gall. As a contributor, by illustration, to the

knowledge, as already made out, he is in some respects unrivalled. His splendid folio atlas, which contains magnificent reprcsentations of the brains and the crania of numberless species of animals, under every set of circumstances, far transcends that of Gall, both in execution and scientific valuc. Moreover, the light which in his *Traité de Phrénologie Humaine et Comparée* he has shed upon some obscure points in phrenology, by aid of comparative anatomy and natural history, is at once considerable and most valuable. Again, the courage, the perseverance, and the disinterestedness with which he has prosecuted his whole career, and more especially the laborious undertaking involved in the production of his great work, are beyond all praise. The author, however, regrets that his commendation, unqualified, can extend no farther. Dr Vimont, he conceives, has very much over-estimated his vocation, manifesting, as he does, at every step, a jealous anxiety to take a rank in the history of phrenology, for which clearly nature never intended him. In every movement, he is goaded by the ambition of *discovery*. This spirit leads him constantly to depreciate the actual value of Gall's labours, and by implication to impeach the accuracy of his habits of observation. He strives perpetually to reduce the results obtained by the great master to the lowest possible point, and this very often quite gratuitously, to say the very least. In the detailed exposition of the physiology of the brain, he has, in almost every point, something either to rectify, or to disprove, or to supersede, or to amplify; constantly falling back upon his vast attainments in comparative anatomy and natural history, from which sources only, he most ostentatiously challenges confutation. Although, in various parts of his work, Dr Vimont recognises, in terms clear enough, the soundness of the method of investigation advocated in the present treatise, viz. the necessity, in attempted *discovery*, of studying a single species, and of comparing the cerebral development of different individuals only where

an identity of elemental type exists, and where all other circumstances are as nearly as possible the same, he has never any scruple in departing from these canons, when he has an idea that he has hit upon something new; and, in such a way, he assumes himself to have discovered several new organs. In the case of *Concentrativeness* and *Inhabitiveness*, he would reconcile Dr Spurzheim and Mr Combe, by making the uppcr half of the presumed organ to be for the latter, and the lower half for the former. Philoprogenitiveness he splits into two, in like manner; the central portion retaining its prescriptive rights, and the lateral portions being assigned to a supposed faculty leading to union for life, in marriage—a faculty apart and distinct from the feelings of attachment and of sex. Then, the questionable organ of Size is not only settled, but is divided into two; one bcing for appreciation of *dimension*, and the other for that of *distance*. He also belicves, or sets forth, that he has discovered an organ of *geometric sense* (organe du sens géometrique), in animals which fly, swim, or march in groups, and always in a definite and regular order. He does not know whether such an organ exists in man or not.

It is true that when Dr Vimont is formally propounding *principles of discovery*, he inculcates the necessity of comparing animals of the same species—of the same parentage even, where practicable, and as much as possible under like circumstances of bringing up; he represents this, moreover, to have been, in some instances, his own proceeding;—but then he never adheres to it, nor feels himself bound by the principle. Vimont's *discoveries* nearly always rest *directly* on comparative anatomy and natural history; the brains of different species arc contrasted for the purpose of detecting the functions of individual parts,—a method, the essential vitiousness of which has already been abundantly shown. As an illustration exemplifying what has just been stated, the instance of the cerebellum may be cited. This structure he makes out to constitute

two separate organs, whereby he, once more, most conveniently reconciles conflicting doctrines,—the doctrine of Flourens and that of Gall. The lateral portions of the cerebellum, or lobes, Vimont allows to be for the manifestation of sexual feeling, but the central, vermiform process, he believes to be for muscular co-ordination. The probable grounds of this latter notion consist in such facts as the following, which he adduces: The cat, the marten, and the squirrel, are instances of great relative development of the middle portion of the cerebellum, and they are distinguished for their muscular adroitness and agility; as, also, the chamois, the goat, the mule, the ass, the horse, and the sheep. "I should not be surprised," says he, "if there should exist some relation between the development of this part of the cerebro-spinal nervous system, and the agility and security of the step in these animals."[1] Certainly, this is no positive conclusion; and, so far at least, it may be conceived that injustice has been done to Dr Vimont, in asserting that, in the practical *details*, he deviates from *principles* recognised by himself in the abstract. Yet how does he expect the matter to be settled? Is it by the institution of comparisons of the relative development of the central portion of the cerebellum with degrees of dexterity and agility in individuals of a single species? Not a word of this is asserted; but another defective method, that of pathology, is affirmed to be the probable source of the future proof. In the concluding words of the particular section he says,—"Pathological facts will one day, probably, clear up this question." And yet the physiology of the central portion of the cerebellum has already been positively proclaimed, by aid of data primarily formed by the phenomena of pathology; and what is the result? Why, M. Serres, having by this means applied himself most especially and assiduously to the matter, maintained,

[1] Traité, vol. ii. p. 242.

whilst opposing Gall, that the part in question (of the whole cerebellum, that alone shut out by Vimont from organic connexion with the sexual instinct) is the *only* portion associated with the amative propensity. Thus to Vimont comparative anatomy reveals one proposition, and to Serres pathology produces one that is diametrically and most remarkably opposed! Behold the unsatisfactory results which come from *fault in method.*

The above details have been presented to the reader only as an imperfect sketch of the functions of the brain, as made out by the application of Gall's method; they have not been offered even as an epitome of Phrenology, in the more extended sense in which the term is most commonly employed. It has not been, nor is it, the author's intention to trench upon the moral and metaphysical considerations which the subject involves, excepting in so far as the realisation of his particular design may be concerned; in the present division of the work, it has been his intention simply to furnish an historical account of the individual organs, illustrating in this way the proceeding of Gall, and exhibiting the general nature of the propositions to which the method leads. Moreover, in supplying the history of the discovery, and subsequent confirmation, of the several details, the author has been anxious to avoid all presentation of the subject in a *systematic* form; and, for this reason, the individual facts and statements have been given, regardless of mere order or arrangement. A contrary course is infinitely preferable in *teaching* a subject whose principles and evidence are undisputed; but when the purpose is to direct attention to the solid basis of fact upon which a disputed doctrine is averred to rest, the exposition of any department of science without regard to *system*, and rather after some such manner as that in which it has been raised, seems better adapted for conveying to the mind an exact idea of its proofs, and of the mode in which these have

been obtained, and must yet be procured. In this view of the case, the writer has first taken the accounts of particular organs just as they have occurred in Gall's own writings; then he has supplied notices of Spurzheim's additions; and, lastly, he has made a brief record of some other propositions which have been advanced by later phrenologists, and in particular by Mr Combe and Dr Vimont. At the close of each notice he has usually given some expression to the convictions of his own mind, as to how far the recorded statements, qualified by his own observations, warrant the inferences generally deduced. And in no other way can Gall's physiology be judged: it is altogether an affair of fact, which observation alone can determine; no profusion of all sorts of knowledge, including every other branch of anatomy and physiology, added to the most eminent powers of reasoning, can deal with this question in the absence of direct experience; no argument, however subtile,—no illustration or analogy, however ingeniously exhibited,—can affect it as a *fact*.

If the things set forth and maintained be a reality, the conclusion is obvious. Unlike what happens in the physiology which erects itself primarily upon vivisections, comparative anatomy, and pathological phenomena, the premises, if sound, admit but of *one* inference—that of Gall and his successors. A constant association and reciprocal influence of structure and function, determine the former to be the organic condition of the latter. If strength in some particular faculty or disposition of the mind be *generally* accompanied by excess in development of particular parts of the brain, and great deficiency in the same structure be *always* followed by feeble displays of the corresponding power, the induction is irresistible, that the cerebral mass is the organ of the associated mental faculty. Deny this; or, like the writer in the North British Review, demand for its proof, before you receive it, evidence that is mathematically rigorous, and you must reject physiology alto-

gether; and, at the same time, you must throw medicine, and all other sciences affecting *vital* phenomena, back again into the regions of mysticism and charlatanism.

Settle, in the first place, the direct question of fact, then reason, mutilate animals, compare anatomical structure in diverse species, conjecture psychical correspondence, give the best explanation you may to morbid phenomena; but decide, first, upon the validity of the phrenological *facts*.

Does not the existing condition of the Physiology of the Nervous System exhibit, in every respect, an analogy to the foregoing exposition of Gall's Physiology of the Brain? Are not the individual propositions affecting the former, like those of the latter, susceptible of classification to some extent, according to their different degrees of validity? For example, certain propositions are received as firmly established; no one doubts, not even Magendie, that the optic nerve is for seeing. It is the same thing with cerebral physiology; all who have observed, recognise the *ascertained* existence of the organ of Self-esteem; those who have not, are without title to speak on the subject. Again, other particulars in Neurology do not obtain an uniform and unqualified assent, as the excito-motory functions in dependence upon speciality of structure; so, in regard to Phrenology, the preceding sketch has set forth several of the details as ranking but in the category of the *highly probable.* In the case of Phrenology, there are some statements advanced whose authenticity is incomplete, and certain inferences which are but *rationally conjectural*, just as obtains with respect to particular divisions of the nervous system,—the Great Sympathetic, for instance. Premature generalisation and groundless speculation have occasionally damaged the career of Cerebral Physiology; who shall declare that the same thing has not happened in the history of the Physiology of the Nervous System?

What would be thought of the philosopher who, in

dealing with Neurology, should set out by affirming a total disbelief in it, without any specification of the items to which he took exception, or on what facts observed he advanced a sweeping negation of the aggregate doctrine? If the grounds of disbelief consisted only of certain general circumstances, bearing collaterally on the question at issue, assuredly we should pronounce him to be of some other school than that of Bacon. In such a case, we should deny to the sceptic the right to any expression of opinion upon the matter, until he had repeated the processes which had led to the particular discoveries, or else had given reasons for distrusting the fitness of such processes for developing solid conclusions. In the instance supposed, we should justly expect the whole, item by item, to be settled by a system of induction as rigorous as such subjects will admit of.

Should it not be so with Phrenology? When its claims are summarily rejected by the physiological sceptic, do we not act rightly in demanding what it is that he objects to? Is it to Phrenology altogether, or to some of its details? or is it to the kind of evidence by which it is supported? State to us the facts which relate *directly* to the question, and that bring about your disbelief, or the reasons why you object to our *method* of investigation. Do this, and we may approach to some settlement of the matter; but, previously, do not scoff at, and deride, or contemptuously treat with indifference, what you do not understand. Take up the propositions, one by one; test their validity by facts parallel to those by which it is asserted that they have been established; and then you will deal with Phrenology as you do with Neurology,—admit what is ascertained truth, doubt, and yet further investigate, what is uncertain, and reject what observation shall decree to be unfounded.

Nevertheless, however clear and certain particular propositions in phrenology may be discovered to be, it is very sure that frequently, in application, difficulties will arise, of a character very likely to shake the conviction as to the

truth of Gall's doctrine, in cases where it rests only upon vague analogy and general facts. But whosoever shall have studied phrenology in the only way in which it can *scientifically* be learned and yet further pursued, will but see in such difficulties, points to be set down for explanation at some future time, when a more extensive knowledge and a larger experience shall have yielded additional light in relation to the subject. This is the course we adopt in kindred departments of science: every day we encounter, in the facts of morbid anatomy, things which to us for a time are inexplicable, and often at variance in their outward seeming with some of the best established doctrines in physiology. The diseases of the spinal cord may serve for illustration of this assertion. Many morbid conditions of this structure did, for a while, look as if directly at variance with the doctrinc finally ratified by Bell. The philosophical physiologist, however, knew that what was rigorously demonstrated could be contravened by no other fact in nature; and, confident in the legitimacy of certain inductions relating to the spinal cord, he calmly awaited the advent of additional light to illumine the temporary obscurity. That light is now being shed; and hereby is afforded a practical lesson, teaching us never to undervalue *direct* evidence because of some *indirect* difficulty. For, assuredly, no collateral fact, nor any circumstance yet to be revealed, however unexplainable it may be for a while, can be shewn to be truly at variance with other facts, rightly determined to be such. Principles once established never become shaken by extended investigations; on the contrary, further support and subsidiary testimony are in this way afforded, by demonstration of the admirable harmony always subsisting between one truth and another. Thus it happens when the arsenals out of which opponents to phrenology take their choicest weapons of attack, are faithfully explored, that these sources supply to it the best secondary testimony of which it is susceptible.

Structural anatomy and mental philosophy, vivisections, comparative anatomy, and pathology, all singularly confirm the very doctrine which they are so often cited to overthrow, by an exhibition of the most striking harmony subsisting between the sure facts which they afford, and the real, not the caricatured, doctrine of Phrenology. To the illustration of this harmony the ensuing chapters will be assigned.

CHAPTER VII.

HARMONY OF CEREBRAL PHYSIOLOGY WITH STRUCTURAL ANATOMY AND MENTAL PHILOSOPHY.

It is obvious to common sense, that when a truth shall have been certified by strict induction, all other circumstances rightly ascertained, will harmonise with it; consequently, if Gall's Physiology of the Brain do actually rest on nature's sure foundations, every other branch of knowledge which exists in any sort of relation to it, must supply corroboration and support by the very circumstances of the correspondence exhibited. And this subsidiary evidence will vary in value and in strength, according as it is drawn from sources more or less nearly allied to the main subject; the more intimate the relation, the more striking should be the demonstrable harmony.

Now, it is quite clear that any exposition of the functions of the brain which is just, must exhibit a doctrine which is closely correspondent with the anatomical structure; at any rate, it is certain that such exposition must involve no proposition which is at actual variance with the known laws of structural adaptation. If the cerebral physiology maintained by phrenologists be sound, it can encounter no insurmountable difficulties in the anatomy; on the contrary, every arrangement of parts of the brain should in some respects elucidate the correspondence subsisting between the structure and the function. A like principle obtains in regard to mental philosophy. If the brain be the organ of the mind, a physiology which rightly

expounds the functions of particular parts, cannot be in discordance with truths which the experience of mankind in all ages has made known, and which have ever been recognised all but universally; and as the association between cerebral physiology and the psychical constitution of man is of necessity so intimate, there should, in accordance with what has been set forth in the premises, be just so much the more facility in deducing secondary proofs of the former, from the certain and well-ascertained facts of the latter.

Phrenology, though made up of the anatomy of the *brain* and the philosophy of the *mind*, is, in itself, something comprised neither in the one nor in the other; but, as a *science*, it is constituted of certain propositions affecting the association—the organic union—of the two. Mind and its philosophy were studied, and, in some points of view, understood; the brain and its anatomy were investigated, and, in many respects, accurately described: these things were objects of inquiry anterior to the epoch of Gall. Yet these two branches of science continued to stand as widely apart as if there were no natural connexion between them. The physiologist did not apply his knowledge of the brain to the elucidation or advancement of mental philosophy; and the psychologist prosecuted mental science as completely oblivious of the brain as if it exercised no influence whatever over mental manifestations. In short, no true physiology of the brain was ascertained; there was no phrenology;—the science which sets forth the laws that affect the mutual dependence and reciprocal influence of mind and brain, and must then, if true, maintain nothing that is inconsistent with the sure circumstances of either. It ought, however, to be remarked, that the observations of Gall, and others, respecting the correspondence of cranial configuration and psychical peculiarity, could not have been invalidated by any revelation resulting from an examination of the interior of the head; the facts, as facts, must have

stood, however little susceptible they should have been of any scientific interpretation. If no parallelism had subsisted between the form of the head, and the figurate surface of the brain—even if this latter had been found to be a semi-fluid, fluctuating pulp—such things would have left the facts in question, in so far as they were rightly observed, absolutely unscathed. Their value, as suggesting a practical inference, could only have been depreciated by the collection of others, similar in their kind, but of a contrary tendency; and, in this way, reducing them to the position of mere coincidences. Gall's observations, however, have not in such a manner been invalidated; yet, had the difficulty alluded to, of affixing to them their true interpretation, been really encountered, we could not have deduced from them the physiology of the brain; their philosophical character would not have been so apparent, and phrenology, as a branch of science now understood, could not have had existence. But if, on the other hand, the outward signs of inward capability result from development of particular parts of the brain, and if this structure be, in all respects, analogous to the constitution of the nervous system at large, in regard to an obvious adaptation to its several purposes, a scientific explanation of the facts accumulated by Gall and his successors becomes practicable. And the very circumstance of the discovery of an anatomy, *after* many of the observations had been made, by which these latter can be interpreted *physiologically*, constitutes one of the strongest corroborations of their essential accuracy, that this, or any other subject, could have received.

Before proceeding to furnish any account of the leading features in Gall's anatomy of the brain, it may be well to call to mind the general constitution of the nervous system, in so far as this is considered to shed light on its functional manifestations. As observed in a previous chapter, the nervous structure is composed of two substances, the white

and the grey, the former being fibrous, and the latter vesicular; the grey matter, which is more vascular than the white, is found abundantly at the centric extremities of the nerves, and also at the peripheral expansion of probably all the sentient nerves; the white, fibrous structure is intermediately placed, forming the proper nervous trunks. Numerous circumstances have rendered it highly probable — not a certainty—that all functional *change* originates in the grey substance, and that the fibrous structure does but *conduct* incident impressions, or centric impulses. The presence of the vesicular matter so constantly at the understood source of power, and at the periphery of the nerves of sense, constitutes the direct evidence in support of this probability, which is strengthened, moreover, by many collateral facts. When much communication and association take place in the functions of different divisions of the nervous system, a corresponding connexion by the white fibrous tissue is observed to exist; apparatus of union being everywhere found, which place one part of the system more or less in relation with the various others. This state of things in the anatomy, however, never revealed function; but this, once ascertained, had its mode of manifestation greatly elucidated by the knowledge of such facts. This circumstance may be exemplified by adducing the visual function and its nervous dependencies. Sight, as a special sense organically connected with a particular portion of the nervous system, was long known; an accurate and extended acquaintance with the anatomy shed light upon the various steps in the process by which vision is accomplished. Thus, rays of light, suitably converged by physical arrangements in the globe of the eye, *impress* the retina composed of vesicular substance; the fibrous structure of the optic nerve *conducts* the impression to a grey, vesicular centre in the encephalon, where the function, as involving consciousness, is considered to have place. Again, if we elucidate

the excito-motory function, by aid of the anatomy (as most generally admitted), we must suppose the *motor* impulse to emanate from the grey substance of the spinal cord, *excited* by incident impressions on the peripheral expansion, in vesicular matter, of the nervous filaments, and *conveyed* to the cord by the white fibres; which kind of nervous substance also *reflects*, eccentrically, the motor influence from within. And the same probable law, as relating to the two kinds of nervous substance, may be applied to the explanation of other offices of the system; the grey matter being properly *functional*, and the white *internuncial.*

How stands the case with the brain? Does its anatomy exhibit any analogy to what we observe in the instance of the nerves, their ganglia, and the spinal cord, in such a way as to display a seeming fitness for the performance of different offices by its different parts? And are the several structures, entering into its aggregate composition, so disposed as to explain the inferences which Gall drew from his physiognomical observations, conformably with nature's apparent plan in the nervous system at large? As the anatomy was uniformly taught previously to the researches of Gall, the reply to these interrogations could hardly have been in the affirmative. For what were the views ordinarily taught, and most generally in vogue, amongst anatomists? Concerning the essential differences between the grey and the white substance of the brain, no uniform or satisfactory doctrine was inculcated. Some anatomists, among whom was Vicq d'Azyr, believed that the grey matter was fibrous in its composition, to some extent. Respecting the white substance, the greatest differences prevailed as to its intimate structure; some maintaining it to be solid, and others tubular. Some conceived both it and the grey matter to be globular; and, whilst some esteemed the white matter to be devoid of blood-vessels, others contended for its being composed entirely of them.

Lewenhoeck, Vieussens, and Steno, recognised its fibrous character; but, anterior to the laborious demonstrations persevered in by Gall, this truth was never very generally received. It is notorious, that when Spurzheim first exhibited the cerebral fibres in this country, the appearances were sometimes attributed by opponents to the scraping of the scalpel, to the impression of blood-vessels, and sometimes to the force employed in tearing the more consistent masses asunder. Indeed, the old anatomical designations of *cortical* and *medullary* substances, scarcely as yet obsolete, sufficiently indicate the opinions that were long entertained regarding the intimate *structure* of the brain. Then, with regard to its several parts;—these were demonstrated, indeed; but who shall say that the proceeding adopted was at all calculated to suggest the rudest idea of function? The grossest notions of mere physical characteristics guided alike the dissections and the nomenclature: mention was made of hemispheres, convolutions, callous body, ventricles, striated bodies, pea-shaped bodies, legs of the brain and cerebellum, writing pen, ram's horns, semicircular tapeworms, pyramidal bodies, and so on. As regards the mode of dissection,—unlike the custom prevalent whenever a notion concerning function has to be illustrated or corroborated by the anatomy, as in tracing parts according to their natural arrangement when dissecting a muscle, a blood-vessel, or a nerve,—the brain was rudely cut up into slices, its fibres, even when recognised, never being traced in their course, but the whole structure was most grossly and *unphilosophically* mangled and destroyed. And the wretched trash resulting from this procedure continues yet, in some places, to form the only cerebral anatomy which is taught. And, certainly, *such* an anatomy would *not* harmonise with Gall's physiology.

Dissatisfied with this state of things, and convinced from several circumstances that just ideas did not prevail with respect to the structure of the encephalon, Dr Gall pro-

ceeded to examine the entire matter anew. After he had advanced considerably in the determination of a more exact and just account of cerebral anatomy, he associated himself with Spurzheim, in conjunction with whom he taught and showed that the brain, in its anatomical constitution, was essentially analogous to the rest of the nervous system, and that, like this latter, it was seemingly adapted to the performance of various functions, so far as this circumstance could be inferred from the anatomy, which tended to elucidate in several respects the physiological views first suggested by the physiognomical observations.

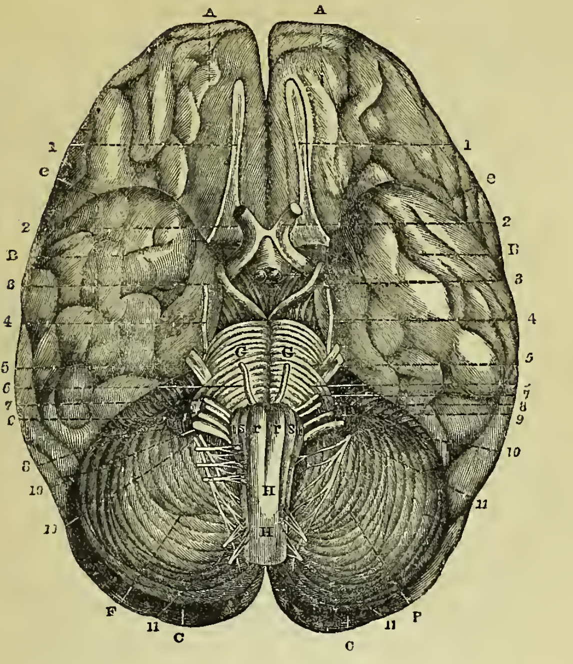
Dr Gall was not an acute observer and philosophical physician only, he was also an excellent anatomist. It must be stated yet once more, that he did not go to the anatomy for the physiology; he did not dissect the brain in order to discover the organs. As will be apparent from details set forth in previous chapters, he ascertained, first, that configuration of the head exhibited the relative development of particular parts of the brain, and that this relative development coincided with peculiarity of thought and feeling; and it was after this determination that he minutely investigated the structure of the encephalon, seeking to make out to what extent there was an appreciable adaptation in the organic disposition of parts to manifest the functions, in accordance with some general plan or design, traceable throughout the whole of the nervous system.

That such an adaptation existed, he felt sure; and, relying with confidence upon the uniformity of mode in which nature brings about analogous results, he did not doubt that this adaptation was discoverable; he was certain that the previous observations on the cranium would, in some way or another, harmonise with the structural arrangements of its inner contents. His researches revealed the cerebral anatomy, a brief epitome of which is subjoined.

The encephalon, comprising in the term the whole of the nervous masses within the head, was traced from below, upwards, in the direction of its fibres ; and not, as by most previous anatomists, by slicing it transversely. In this way, a method was employed exactly correspondent with what had been, and continues to be, practised in the case of the nerves, muscles, blood-vessels, and other textures. The commencement of the encephalic fibres at the superior extremity of the spinal cord was recognised, and their course pursued upwards to their peripheric expansion in the grey matter of the convolutions; just as we proceed with a nerve in examining its anatomical relations physiologically, we begin at the centric origin, dissecting along the course of the fibres, until we attain its expansion at the periphery.

The ensuing account of the direction of the cerebral fibres, and their relation to the convolutions superiorly, and the medulla oblongata inferiorly, will be rendered tolerably clear and intelligible by aid of the following cuts. They and the explanations are taken from the Introduction to Mr Combe's Translation of Gall on the Cerebellum. Fig. 1, represents the base of the brain and the cerebellum.

Fig. 1.



$\left.\begin{array}{l}\text{AC}\\\text{AC}\end{array}\right\}$ Are the right and left hemispheres of the brain.

FF, The cerebellum.

AA, The anterior lobe.

e e, The line which denotes the separation between the anterior lobe and the middle lobe.

BB, The middle lobe.

CC, The posterior lobe.

GG, The *Pons Varolii*, which brings the two sides of the cerebellum into communication. It is also named the *Tuber annulare.*

HH, The *Medulla oblongata.*

r r, The *Corpora pyramidalia.*

s s, The *Corpora olivaria.*

t t, The *Corpora restiformia.*

1. First pair, or olfactory, arise by three origins. These unite and proceed forwards and inwards in a groove in the inferior surface

of the anterior lobes of the brain, and form a greyish swelling or ganglion. From this ganglion a great number of filaments proceed through the cribriform plate of the ethmoid bone, and are distributed upon the mucous membrane of the nose. It is the nerve of the sense of smell.

2. Second pair, or optic, arise principally from the anterior *corpora quadrigemina.* Each nerve passes outwards through the optic foramen in the sphenoid bone, and is expanded upon the retina. It is the nerve of the sense of sight.
3. Third pair, or *motores oculorum,* originate from the motor tract of the spinal chord, immediately after they have passed through the *pons Varolii.* Each nerve escapes through the sphenoidal fissure, and supplies five of the muscles within the orbit with motor filaments.
4. Fourth pair, or *trochleares,* originate from the *processus è cerebello ad testes* and *valvula* of Vieussèns. Each nerve passes out from the cranium at the sphenoidal fissure, and is entirely distributed upon the superior obliqne muscle of the eyeball. It is a motor nerve.
5. Fifth pair. These nerves issue from the surface of the brain, near the junction of the *pons Varolii* and *crus cerebelli,* but actually arise from the restiform bodies. Each nerve escapes from the cranium by three separate openings, and is extensibly distributed upon the orbit and other parts of the face. Part of the filaments of this nerve are *sensitive,* and part *motor.*
6. Sixth pair originate from the pyramidal bodies, as they are abont to enter the *pons Varolii.* Each nerve escapes through the sphenoidal fissure, and is entirely distributed upou the external rectus muscle of the eye-ball. It is a motor nerve.
7. *Portio dura* of the seventh pair originate from the restiform bodies. Each nerve is extensively distributed in the muscles of the face and external ear. It is the motor nerve of the muscles of expression of the face.
8. *Portio mollis* of the seventh pair, or auditory nerves (eighth pair of some authors), arise principally from a small grey swelling on the upper surface of the restiform bodies at the side of the fourth ventricle. Each nerve is distributed upon the internal ear, and is the nerve of the sense of hearing.
9. Glossopharnygeal nerves, or upper division of the eighth pair (ninth pair of some authors), arise from the restiform bodies near the sulcus which separates them from the olivary, and are distributed upon the pharynx and mucous membrane at the back part of the tongue. They are sensitive nerves.
10. *Par vagum,* or pneumogastric nerves, or principal division of the eighth pair (tenth pair of some authors), originate in the same line with, and close upon, the glossopharnygeal. These nerves are extensively distributed upon the larynx, pharynx, trachea,

œsophagus, heart, lungs, and stomach. Part of the filaments of this nerve are sensitive, and part are motor.

11. Spinal accessory nerves, or lower division of the eighth pair (eleventh pair of some authors), originate from the upper part of the spinal chord, in the same line with the two preceding nerves. They enter the cranium by the foramen magnum, and pass out again from the cranium through the foramen lacerum, along with the other two divisions of the eighth pair. They are principally, if not entirely, motor nerves.
12. Hypoglossal or ninth pair (twelfth pair of some authors). Each originates from the sulcus between the pyramidal and olivary bodies, and escapes from the base of the cranium through the anterior condyloid foramen, and is distributed upon the muscles of the tongue. It is the motor nerve of the tongue.

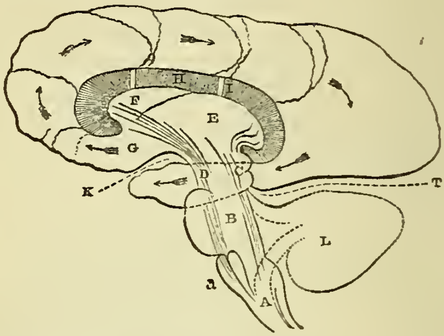
The next figure represents a perpendicular section of the interior of the brain, not far from the middle line, proceeding from the surface of the convolutions down to the spinal cord. In this cut, the darker portions represent the vesicular structure, and the white radiated part displays the general character of the fibrous portion.

Fig. 2.

e e, Is a section of one of the *corpora restiformia.*
c, Is a section of one of the *corpora pyramidalia.*
b, Is the *pons Varolii.*
g, Is one of the *crura* of the brain.
34, 35, 37, 38, and 11, Are the cerebral fibres, which, originating in the *medulla oblongata,* as after described, pass under the *pons Varolii,* through the *crura,* and *corpora striata,* and *thalami nervorum opticorum,* and ultimately expand into the convolutions of the brain.
47, 48, Situation of the cerebellum within the skull.

A third figure is introduced, which shows the situation of the *tubercula quadrigemina,* and some other parts, frequently referred to in the present work.

Fig. 3.



A, Represents the *medulla oblongata.*
a, *Corpus pyramidale.*
B, Pons Varolii, or tuber annulare.
C, Tubercula quadrigemina, with the fibres of the posterior columns passing in front of them.
D, *Crus cerebri,* with some of the fibres of the anterior columns. These fibres are more fully shown in fig. 2.
E, The thalamus nervi optici of one side.
F, The anterior corpus striatum.
G, Substance of the hemisphere springing out from the front of the anterior corpus striatum.
H, Space between the corpus striatum and the hemispheres, caused, in this figure, by the introduction of a small piece of wood.
I, The two surfaces, being in contact in the natural state.
K, Fissura Silvii.
L, The cerebellum.
T, The tentorium, separating the cerebellum from the brain.

At the upper extremity of the spinal cord, the fibres continuous with the motor tract run into the *corpora pyramidalia*, situated on the anterior portion of the *medulla oblongata*. These *corpora* are mainly composed of white fibres, which decussate at their lower extremity (H of fig. 1.); they then proceed upwards through the *pons varolii* (G G of fig. 1.); emerging from its upper border, the greater number pass still upwards, forming the anterior and external bundles of the *crura cerebri* (g of fig. 2.), and the exterior part of the *corpora striata;* and, finally, they expand into the inferior, anterior, and exterior convolutions of the anterior and middle lobes of the brain. Gall maintains, and it is generally allowed, that there is an observable proportion between the size of the pyramidal bodies, and the convolutions in traceable connexion with them.

A portion of the fibres of the *corpora pyramidalia* pass into the *thalami optici* (erroneously so called); these, the principal ganglia of the posterior and middle lobes of the brain, as they were taught to be by Gall, contain much grey matter, supposed to yield reinforcement to the fibres passing through them, which ultimately expand to form the posterior and some of the middle cerebral convolutions.

A number of fibres from the pyramidal bodies near the point of decussation, accompanied by certain fibres from the motor tract of the cord, proceed to the cerebellum.

If this account of the final distribution of the constituent fibres of the *corpora pyramidalia* be correct (and that it mainly is so, there is no question), we have these bodies chiefly in relation with the anterior cerebral lobe, but to some extent also with the hemispheres at large, and with the cerebellum.

A connexion between the pyramidal bodies and the posterior and middle, as well as the anterior portions of the *cerebrum*, has recently been confirmed and elucidated by the researches of M. Foville.[1]

[1] A brief account of Foville's anatomy in this particular occurs in the

The fibres of the *corpora olivaria* (S of fig. 1) pass upwards into the *pons Varolii*, and form the posterior and interior parts of the *crura cerebri;* from these they proceed

last—the sixth—edition of Mr Combe's Elements of Phrenology, supplied by Dr John Reid, the learned and scientific professor of St Andrews. It is as follows:—

"Foville represents the ascending fibres of the anterior column of the *crus cerebri* (motiferous column) as proceeding to the whole of the convex and upper surface of the brain, even to its posterior extremity. See his Plate 18, N N, *n n*. He describes the connexion of the sensiferous column as follows:—The ascending fibres of this column, G L, in his plate, are seen passing onwards to the *substantia perforata anterior*, and becoming connected with the olfactory and optic nerves attached to this part of the encephalon. These sensiferous fibres also form the medullary covering of the floor of the lateral ventricles. The connexion of the sensiferous fibres with the convolutions of the hemispheres, according to Foville, takes place in the following manner:—The grey layer placed upon the surface of the convolutions, and forming their periphery, consists of alternate layers of grey and white medullary matter. Proceeding from the surface inwards, these layers are arranged (parallel to the external surface) as follows:—1st, medullary or white; 2d, grey; 3d, white; 4th, grey; 5th, white; 6th, grey. This last layer of grey is placed on a layer of white. These white layers, entering into what is called the grey matter on the surface of the brain, are continuous with the lining medullary membrane of the floor of the lateral ventricles; in other words, with the sensiferous fibres. If this view be correct, the sensiferous fibres extend in the form of a medullary expansion in contact with the grey matter, over the surface of all the convolutions of the brain. This connexion between the medullary layers in the cortical substance on the surface of the hemispheres and the sensiferous fibres of the medulla oblongata, takes place at the *substantia perforata anterior*. This part of the brain, to which are united the only two cerebral nerves—the optic and olfactory—and from which emanate, as from a centre, all the convolutions on the surface of the brain, has not been sufficiently attended to by anatomists.

"According to Foville, it is composed of a substance of a peculiar kind, differing from all other parts of the brain, perforated by innumerable vessels united to, surrounded and enclosed by, the roots of the above-named nerves, which cover and penetrate it by prolongations from their own substance, and is situated in the deep central part of the basilar region of the fissure of Silvius. It is of a quadrilateral form, placed below the insertion of the fasciculated part of the *crura cerebri* (peduncle) in the substance of the brain. It is separated from the peduncle by the *tractus opticus*, which is attached to its posterior

through the great posterior ganglia (*thalami optici*), and then expand, partly into the convolutions of the anterior lobe that lie on its superior surface towards the middle line, partly into the superior convolutions towards the central line of the middle lobe, but chiefly into the convolutions of the posterior lobe.

Some anatomists assert that a decussation of the fibres of the olivary bodies takes place at the same point as that where the *corpora pyramidalia* decussate.

The fibres of the *corpora restiformia* (*t t*, fig. 1) ascend and form the principal part of the cerebellum; a portion, however, pass onwards, and enter into the constitution of the posterior cerebral convolutions.

In the centre of the *crus cerebri*, the fibres from the *corpora pyramidalia* (D, in fig. 3) are separated from the fibres of the posterior division of the *medulla oblongata* (to the left of the letter C, in fig. 3) by a portion of darkish grey substance called *locus niger*.

border. But, for a more particular description, we must refer to Foville, *Anatomie*, p. 181.

"The radiating fibres of the motiferous columns of the medulla oblongata occupy the centre of the hemispheres, send ramifications into all the convolutions on the lateral and upper surfaces of the hemispheres, and penetrate the internal layers of the cortical substance covering the external surface of these convolutions.

"The radiating fibres of the motiferous column of the *crus cerebri* have, therefore, their peripheral extremities covered by the expanded layers of the sensiferous column, which are intermixed with and form a part of the grey cortical substance on the surface of the convolutions.

Foville, in his *resumé* of the structure of the brain, given at p. 487 of his work, states that the cerebral prolongations of the *posterior column* occupy in this organ the situation which the skin and mucous membranes do in the body; and upon these two tegumentary membranes of the body numerous branches of the sensiferous nerves connected with this posterior column are ramified, while none of the motiferous nerves reach it.

"The cerebral prolongations of the *anterior column*, contained in the interspace between the membranous expansions of the posterior column, occupy in the brain the place which the muscular system, animated by the nerves attached to the anterior column, holds in the body."

The two hemispheres of the brain, as well as its several subdivisions, are brought into mutual communication by means of structures called *commissures*, composed chiefly of white fibrous tissue. An excellent classification of these is supplied by Mr Solly, in his work on the Brain, under three heads,—the transverse, longitudinal, and oblique.

The *transverse* commissures are the following:—

1. The great transverse commissure of the hemispheres, the *corpus callosum.*
2. The *pineal* commissure.
3. The posterior commissure, or commissure of the posterior cerebral ganglia.
4. The soft commissure, also of the posterior ganglia.
5. The anterior commissure, or commissure of the anterior cerebral ganglia (*corpora striata*).
6. The commissure of the cerebellum, the *pons Varolii.*

The *longitudinal* commissures are two:—

1. The superior longitudinal commissure.
2. The inferior longitudinal commissure, the *fornix.*

There is but one *oblique* commissure, commonly called the *processus è cerebello ad testes*, with the *valve of Vieussens.*

CEREBELLUM. This organ, in description, is commonly divided into two portions,—the *lobes*, or lateral portions, and the *vermiform process*, or central part. In the formation of the cerebellum, the greater part of the fibres of the corpora restiformia run into it, and upwards in the direction of certain vesicular matter forming the proper ganglion of the cerebellum, usually denominated *corpus dentatum;* having attained this structure, they become inserted in it, and emerging greatly enforced, they expand, finally, to form the lobes, in the same way as the fibres issuing from the great cerebral ganglia (*corpora striata* and *thalami optici*) go to form the hemispherical convolutions.

Certain fibres arising from the summit of the anterior column of the spinal cord, and from the inferior extremity of the *corpora pyramidalia*, proceed, as before stated, in the

direction of the cerebellum, and enter its substance, bringing the organ into anatomical connexion with the motor division of the cord. The existence of this connexion is alluded to by Gall and Spurzheim, but Mr Solly seems to have been the first who clearly demonstrated it.

The Pons Varolii is to the cerebellum what the *corpus callosum* is to the cerebrum, its great transverse commissure.

Corpora Quadrigemina. These four little eminences (as they exist in the mammalia), seem to be in especial connexion, by fibres, with the *corpora olivaria;* some fibres, moreover, from the pyramidal bodies run also to them.

The preceding description of the encephalic structures has been adduced only as furnishing materials for an estimation of the analogy which subsists between the true anatomy of the brain, and the recognised constitution of the nervous system. Although it can, of course, make no pretensions to a full anatomical exposition, it may yet be sufficient for the purpose of recalling to the mind of the student the relations which obtain among the various parts of the encephalon, as also its general constitution. The presence of grey substance at the extremities of the white fibres, displays a striking correspondence with the anatomy of the nerves; and the analogy will also have been still further apparent in the mode in which the cerebral masses are brought into reciprocal communication. This being accomplished, both in the case of the brain and of the nerves, by intercommunication of fibres among the several parts.

The anatomy of the brain, as made out by Gall, and first taught by himself and Spurzheim, is now but very rarely disputed, although for a while it was ridiculed and denied. Afterwards, when its justice and fidelity could no longer be concealed, the prejudices that existed among many persons against its real author, caused it to be attributed to Reil, and to almost any anatomist who, soon after it was first propounded, gave to it a sanction by teaching it at second

hand. Now, however, that time has done its usual work, and mitigated early hostility, its true origin is very generally allowed, and an impartial posterity will not fail to award to Gall the great merit, which is his own, not only of discovering the true physiology of the brain, but also of having been the first rightly to make out its intimate structural constitution. It is not meant that no errors in detail are to be found in Gall's descriptions, or that every appearance which he noticed, received a correct interpretation. It must be maintained, however, that all the leading features of his anatomy are substantially accurate, and that they exhibit, moreover, an adaptation to function in the encephalon on a plan that is common to it and the nervous system at large, by analogous disposition of the vesicular and the fibrous substances, and by reciprocal communication of its various divisions; and that, in this way, the brain and its physiology, are in harmony with all our well established notions concerning the fitness of structure for the manifestation of function.

It has been already shown, in the present chapter, how the exercise of function, on the part of individual nerves, or systems of nerves, harmonises with their physical arrangements; how grey matter receives the impressions *ab extra*, which are conducted by white fibres to a grey ganglionic centre, where the specialities of function are supposed principally to have place. A precisely similar explanation may be developed in regard to the brain, by admission of Gall's anatomy and physiology.

The convolutions of the hemispheres appear to stand in a relation to the ganglionic centres of the nerves of animal life, very analogous to that in which these centres themselves stand to the superficial expansion of their respective nerves. Thus the nerves of special sense, and of common sensation, terminate all in grey matter at the base of the brain. This grey matter is universally in communication with the white fibrous structure which runs into the convo-

lutions, again composed largely of grey matter. Now, when an impression is made upon the senses, as when objects are seen; when articulate sounds are heard; or when the body is bruised; the sensible impression made upon the ganglionic centre acts upon the intellect, located in the anterior convolutions, and it may also, and very generally does, exert an influence upon the feelings; and these effects are explained by the existence of communicating fibres. In many cases, the impression gives rise to some action accomplished by the muscular system, through the instrumentality of motor fibres, connected, through the pyramidal bodies, chiefly with the intellectual compartment of the brain, whence proceeds *Will.* Very often impressions on the sentiments and propensities induce visible effects on the muscular system, in emotional expression and gesture, independently of all volition, and at times even in opposition to it; and, in correspondence with these circumstances, modern researches have exhibited, as before stated, a fibrous connexion between the motor strands of the spinal cord and the encephalic organs of the feelings, as discovered and taught by Gall,—a connexion which supplies all necessary data for the formation of a reasonable and highly probable theory of instinctive and emotional movements, by assumption of a reflex influence upon the motor system, inherent in all the central ganglia, and brought into operation by incident impressions.[1]

In all mental phenomena, the intellectual faculties, the moral sentiments, and animal propensities, act and react

[1] The author has great pleasure in referring the reader for some curious and interesting points in relation to this subject, to an able and valuable paper by Dr Laycock, published in the British and Foreign Medical Review for January 1845, " On the Reflex Functions of the Brain." Also to a remarkable correspondence between that gentleman, Mr Combe, and Professor Reid, published in consecutive numbers of the Lancet, during the months of August, September, and October 1845. Mr Combe, in the last edition of his " Elements," has applied Dr Laycock's theory, with very beautiful effect, to the whole subject of phrenology.

one upon the other, displaying reciprocal influence in all sorts of ways; and, correspondent with such a state of things, the several masses of both grey and white matter are brought into mutual communication by the general fibrous arrangement of the structure and by the commissures. These views might be considerably extended; it would, however, be beyond the general purpose of this work, to do more than briefly to indicate them. The whole subject, in this point of view, is most ably and fully discussed, in the publications just referred to in the note.

An objection to Gall's physiology is sometimes deduced from the anatomy of the brain, because mechanical divisions corresponding with the organology do not exhibit themselves,—mapping out the encephalic surface after the manner of the phrenological bust; and some persons would appear to reject phrenology, or at least to disregard it as a scientific pursuit, on this very account. In a course of popular lectures on physiology, recently delivered by a distinguished metropolitan professor, in the town in which the author writes, the lecturer adduced excellent reasons for preferring the mental philosophy evolved by phrenology to every other,—admitted the general correctness of the physiognomical observations,—avowed himself, moreover, to have been for some years a member of a phrenological society, with a view to satisfy himself of the amount of truth contained in the subject; and yet this professor concluded by asserting that "it does not appear to be altogether sustained by the anatomy," suggesting, in this way, that, so far as true, phrenology is but a species of improvement on the system of Lavater. Again, in a paper published in the Edinburgh Review, July 1845—the authorship of which is imputed, with every probability, to Professor Sedgwick—the writer, reviewing "Vestiges of the Natural History of Creation," objects to Gall's doctrine, not because the observations are too scanty or inaccurately made, but because it is "unsustained by any direct anato-

mical proof;" and the reviewer further states, "We have several times seen the human brain dissected (and twice by Dr Spurzheim himself), and we affirm that neither he nor any one else has been able to demonstrate any subdivision of its structure, corresponding to the organ theory."

Objections such as these can only arise from singular thoughtlessness, or from utter ignorance of nature's plan in the development of the nervous system, which, *as a rule, exhibits no mechanical divisions coincidently with distinctness in function*;—if it were otherwise, there might be some plausibility in the objection, although its conclusiveness could not be admitted, seeing that, in the case of the brain, the specialities might be such as to demand some deviation from the customary method. But, in point of fact, the physical arrangements of the nervous system accord with no such mechanical prescriptions, either in man or in animals. The trunks of the nerves are composed of filaments in subservience to various functions, and the several filaments are absolutely undistinguishable in their relation to function: not only is this the case, but the grey masses of ganglionic substance at their origin frequently defy all attempts to discriminate lines of demarcation coincident with functional differences. If we take the spinal cord itself for examination, we shall certainly discover furrowed lines on its various sides; but there is no reason whatsoever to believe that these correspond with differences in office; indeed, the evidence is all the other way; it is pretty certain that they do not. The roots of the motor and sensitive nerves, in their connexion with the cord, do not come from mechanically-divided portions; for they issue, in both cases, from the antero-lateral strands; one root originating behind the other, but in each case anterior to the lateral furrow. The medulla oblongata is linearly divided, but no certainty exists that such division is altogether coincident with distinctness in function. Certain filaments of nerves emanating from vesicular structure,

wherein no marked division obtains, undoubtedly have plurality of function; and when doubts are expressed with regard to this fact in some particular case, they are not caused by the absence of lines of demarcation. Many esteemed physiologists think that the glosso-pharyngeal nerve has more offices than one—an inference that is deduced from the supposed manifestations, and not from a recognition of mechanical divisions. It is the same thing with the entire nervous system. Why, then, should it have been assumed that a different plan was necessary in the instance of the brain? If it were not matter of constant experience to what utter perversion of all sound reasoning prejudice leads, it would be impossible to conjecture how this should have happened. As the case stands, it is very probable that such an argument originated in some determination to *put down phrenology*, and that it has been kept up from its obvious fitness for captivating the superficially informed.

Nevertheless, it may be asserted that such lines of demarcation as do permit themselves to be noticed in the ganglionic origins of nerves, and in the spinal cord, are constant, and admit of ready identification; suggesting, under these circumstances, that differences of function *do* exist, however little these may correspond with the mechanical divisions; but that, on the other hand, the furrows indicative of distinctness in the cerebral convolutions, are in no regular order, whilst every thing looks more like accidental packing up, than a settled anatomical arrangement. This is an old objection, not yet altogether abandoned; and, for a long time, it was deemed to be of serious import. When Dr Spurzheim denied the premises, contending for the fact as existing differently, and when he maintained that order and symmetry obtained as completely in the co-ordination of the convolutions of the brain, as in other parts of the nervous system, his position was controverted, and sought to be demonstrated in the nega-

tive. Yet, how stand the actual circumstances of the case? Why, the fact is, that, at the present day, no manner of doubt remains that the disposition of the cerebral convolutions is upon a plan that is systematic; every anatomist of any repute admits it to be so; and if individuals will but consult nature's book impartially, they will discover it to authorise the statements which affirm the fact.

Recently, M. Leuret, an opponent of phrenology, has shown that, by comparing the most complex and highly convoluted brains with others in which the folds are few and simple, the convolutions of the latter, which, in their mode of disposition, form the primitive type, may be demonstrated in each successive group of brains, as they constitute, seemingly, a *rest* in the ascending scale. Dr Todd and Mr Bowman, who cannot be charged with any predilection for the physiology of Gall, do yet, in the following terms, corroborate his anatomy, in its relation to the arrangement of the vesicular structure of the hemispheres: they say—"When the brain has acquired an enormous size, as in the elephant and in man, new convolutions seem to be added to the primary ones met with in inferior groups, and the secondary folds are greatly increased in number. The additional convolutions are found chiefly at the superior and anterior part of the hemisphere." [1]

Some persons have contended that, although phrenology, according to external indications, makes out a double brain with symmetrical disposition of the organs, the brain actually shows no such symmetry in the arrangement of convolutions in the two hemispheres. Now, every work on phrenology, possessing the slightest pretensions to scientific merit, admits that, very often, the development on the two sides of the head is unequal, but that, nevertheless, the essential organs exist symmetrically, just as it happens with other structures in the two halves of the body. The

[1] Physiology of Man, pp. 281–2.

special senses, the component parts of the face, the voluntary muscles, and some other organs, are found upon each side of the mesial line with a general development and arrangement alike on both halves of the body; but the symmetry is neither absolute nor uniform. How far does all this correspond with our present knowledge of the cerebral convolutions? The results of the researches of Dr Todd and Mr Bowman, the latest writers of any authority upon the subject, are set forth in the following terms:

"In man, the convolutions of the right and left hemispheres do not present a perfect symmetry. It is important, however, to notice, that careful examination will invariably display the same essential convolutions on each side, although they present such striking differences in detail, that it is at times difficult to recognise the likeness; and it is not a little remarkable, that, in general, the lower the development of a brain, the more exact will be the symmetry of its convolutions. Thus, the brains of all the inferior mammalia, even of those which make the nearest approach to man, are exactly symmetrical. The imperfectly developed brain of the child exhibits a similar symmetry; and that of the inferior races of mankind, in whom the neglect of mental culture, and habits approaching those of the brute, are opposed to the growth of the brain, also presents a symmetrical disposition of the convolutions."[1]

It is, indeed, certain, and placed beyond all doubt, that, whatever be the structural adaptation for the performance of different functions in the several ganglionic centres of the nervous system at large, a corresponding state of things is discoverable in the cerebral convolutions, and an analogous disposition of the grey matter. Order and symmetry characterise the presence of this latter everywhere, both in the nerves and in the encephalon; but, so far as the evidence yet goes, furrowed lines do not appear to mark the

[1] Physiology of Man, p. 283.

the structures coincidently with division in their functions. This is admitted with regard to the nerves; why should a different plan be anticipated in the case of the brain? If a necessity for such deviation from the general scheme be in question, the *onus probandi* lies not with the phrenologists who see no need for it, but with such writers as the Edinburgh Reviewer of "Vestiges of the Natural History of Creation."

Reference has already been made to Foville's Anatomy of the Brain, which, coincidently with some other researches of recent date, sets forth that all parts of the cerebral hemispheres are in direct fibrous connexion with the motor division of the spinal cord; and it has been maintained that this fact harmonizes well with certain phenomena involved in emotional actions of the muscular system, which, so long as it appeared that the motor tract was only in communication with the anterior cerebral lobes, were somewhat difficult of explanation. The author, however, has heard it objected, that from the inherent difficulties of the investigation no such *separate* and *distinct* connexion can be made out between various parts of the hemispheres and portions of the spinal cord. In reply to this it is to be observed, that it may indeed be an impossibility to trace every fibre in all parts of the brain through the *optic thalami*, and *corpora striata*, beneath the *pons varolii*, along the *crura cerebri*, down to the divisions of the *medulla oblongata* and cord: all that is practicable is to show that fibres from the lower structures ascend, and run continuously into some of the fibres demonstrable in the higher. It is a sure physiological inference that the sensori-volitional fibres, distributed throughout the body, have a connexion with the medulla oblongata; but there is no possibility of tracing every individual nervous fibre, through the trunks, to the spinal cord, and up to the medulla: if every fibre was *separately* and *distinctly* continued into this latter structure, its volume would far exceed that which we witness. The

fact stands thus:—A connexion, to some extent, *can* be made out between the motor fibres of the cord, for example, and the pyramidal bodies, through which latter, the will transmits its influences to the muscles from the anterior lobes of the brain. To gain the recognition of this inference, it is not thought necessary to demonstrate that the pyramidal bodies, or that the anterior division of the upper segment of the cord, include by continuation the aggregate of all the volitional nerves. Any difficulties, therefore, which may arise in reference to these matters, from imperfect demonstration of the communication subsisting between the several convolutions of the brain and the motor strands of the spinal column, are but of a character with that which obtains in the whole structural anatomy of the nervous system.

If the anatomy of the brain so strikingly harmonise with Gall's physiology, how does the mental philosophy which it developes correspond with the daily experience of human nature, as recorded by metaphysicians, moral philosophers, and theologians? As the author's present object is almost altogether physiological, he will enter but very slightly into this topic, and this with the view rather of giving a sort of completeness to the illustrations of the evidence attesting the soundness of Gall's physiology of the brain, than with an intention of entering into any full discussion of the matter in its moral bearings. He will, therefore, revert only to some leading circumstances calculated to exhibit the harmony and correspondence in question.

The physiology discovered by Gall renders it a matter of scientific demonstration, that special talents and particular dispositions are innate; that *genius* is born, and not created by education; that *goodness of heart*, and other distinctive properties of the inward *feeling*, are not accidental circumstances, but that, radically, and in great part, they are dependent upon the natural constitution of the individual. Further, it is rendered demonstrable, that the va-

rieties which are to be observed in talent and disposition come, in like manner, from nature's primitive ordination. As exemplified in the foregoing chapter, one person shall have one kind of talent in a high degree, and be distinguished for nothing besides; whilst another person shall be remarkable, in the same way, for some other talent only: one individual shall reason most acutely and profoundly, and yet be no linguist, in spite of efforts that may have been made to overcome the defect; in some other instance, the exact converse of this will be found to have place. As regards the moral disposition: whilst one person shall ever feel kindly towards his fellow-creatures, another shall be almost an entire stranger to the sentiment of benevolence, and yet be rigidly just: these different qualities shall have displayed themselves in their respective subjects *ab initio*, although, essentially, the outward training shall have been the same. All this is sure; and phrenology places it beyond mere opinion, because it reveals organic conditions which are congenital, and coincident with such psychical diversity.

Does not such a state of things harmonise with our constant experience? Would it ever have been doubted in an absence of all *speculative* philosophy? Every one who is conversant with the nursery, with schools, or with childhood and youth, under any set of circumstances, must laugh to scorn the theories of the closet. Let three children, all of the same parents, and brought up under like circumstances, be regarded; one shall surpass its fellows prodigiously in the faculty of learning by heart; another shall be remarkable for arithmetical ability, obtaining results *con amore*, which may have cost the other two much and difficult appliance of the mind; and the third shall be distinguished for neither of these capabilities, nor for any other particular aptitude for learning, but, nevertheless, shall, in spite of some seeming dullness, strike an attentive and reflective observer, by undoubted indications of *thought*

beyond its years, in asking *old-fashioned questions*, and in seeking perpetually for *reasons* rather than for mere *information.* To pursue the illustration: differences of disposition shall be equally remarkable in the supposed instances, with the intellectual peculiarities; one shall be good-natured, another selfish, and the third vain and conceited, and all this in complete absence of any diversity in the outward circumstances; proving that the differences are innate. Cerebral physiology, as made out and taught by Gall, harmonises with all such facts (more or less within every one's experience), inasmuch as it shows that the organization which, in our earthly existence, manifests mind, exhibits diversity in constant correspondence with this variety in the talents and the dispositions.

However metaphysicians may have differed among themselves respecting analysis and classification, they have very generally recognised the fact of plurality in the mental powers, and of their diversities of strength in different individuals. "The varieties of intellectual character among men," says Mr Stewart, "present another very interesting object of study, which, considering its practical utility, has not yet excited, as much as might have been expected, the curiosity of our countrymen."[1] Merely contemplative students do not, however, always regard these varieties as dependent upon the innate constitution; yet that they are so, in great part, is a conclusion of experience; and cerebral physiology puts the question beyond a doubt. Moral philosophers, it has been shown in a previous chapter, admit a like state of things in the case of the feelings, susceptible of a physiological explanation by the discovery of Gall. Divines have, in all ages, taught the existence of variety in the gifts and talents of mankind, in illustration of the doctrine that much is required where much is given. Diversity in temptation and facilities for particular virtues,

[1] Dissertation, Encyclopædia Britannica, vol. i. p. 222.

according to natural constitution, are familiar to the thoughts of moral theologians and of casuists. How beautifully do all these things correspond with their dependence, in some measure, upon the natural constitution of the cerebral organisation! Who have ever been regarded as the great masters in the knowledge of human nature, in spite of the obloquy which, justly or unjustly, they have so often sustained? Who, in their educational capabilities, have long been celebrated as a class? Why, the Jesuits, whose distinctive proceedings in dealing with the education of youth, have always consisted in the acuteness and assiduity with which they have determined specialities in the individual, in reference alike to intellectual capability and to moral tendency. The Jesuits, both by enemies and friends, are reputed to have studied mankind most successfully, and the results, in their practical bearings, harmonise most remarkably with the physiology of Gall. Here may also be mentioned, the notorious fact, that certain faculties display a prominent activity and power, only at particular seasons of life—a circumstance utterly inexplicable excepting on the theory that different faculties act through separate organs. Indeed, in whatsoever way we should propose to demonstrate the harmony which subsists between the true physiology of the brain and a sound philosophy of mind, the difficulty occurs, not in the discovery of appropriate illustrations, but in a selection of such as may appear the most striking; for every where it is seen that difficult problems affecting the human mind receive their best, and in some cases, their only *scientific* elucidation in this physiology of Gall.

The existence of native differences in particular talents and dispositions, is a *fact*. It is just one of those things which mankind takes for granted: ere it is doubted, sophistical speculation must have done its work. All the views and schemes we form regarding our fellow men, rest essentially upon this truth; and the moral consequences flowing

from it, have ever been deducible; the additional truth that recognisable specialities in the organisation, exist in coincidence, constitutes no novelty in respect of *tendency;* the moral truth itself is absolutely unaffected by the discovery of phrenology. In all these things there is harmony, as in the rest of God's works. One truth is sustained by another; moral philosophy and physiology are in concord and unison. Be assured that, whatever be the moral bearings of Gall's physiology, they are not new; and the present writer scruples not to affirm that, when conclusions are deduced, by particular writers, from phrenology, which, as is sometimes the case, shock the general sense of the community, they have not been formed by phrenology in fact, but have existed in the mind, in germ at least, quite irrespective of it. Men, from totally different premises, lean to certain notions regarding morality and religion. When they become acquainted with phrenology, they use it, as they use *all* their collateral knowledge, to corroborate and to develope still further, their previous ideas.

When attentive consideration is bestowed upon the harmony subsisting between certain physiological facts and correlative mental phenomena, it is impossible not to recognise, in such a circumstance, a powerful corroboration of the truth of the doctrine by which it is developed. Indeed, so potent and irresistible has this sort of evidence been felt to be by large numbers, that Gall's physiology has often been admitted on account of its mere force, without any direct examination of the facts of the case. It has been maintained, that, in the main, phrenology must be true, as on any other supposition so admirable and so practical a system of mental philosophy could not have emanated from it. Many most esteemed writers on other subjects have avowed, that no analysis or classification of the mental faculties, is at all comparable with that of the phrenologists; and others, less candid, will be found to draw largely from phrenological sources, without acknowledg-

ment. In many of these cases, no thought may have been bestowed upon the organology; but parties, having discovered a mode of specifying and arranging the mental faculties, singularly apt for explaining the phenomena of mind, as encountered in the constant experience of mankind, avail themselves furtively of it, not having the ingenuousness to avow themselves *pro tanto* phrenologists. It is really amusing, sometimes, to read medical and educational disquisitions which have gained the best part of their reputation from the unacknowledged use made of phrenology; their authors having discovered that ideas which strictly flow from it, are sometimes accepted the more readily, if sufficiently disguised to conceal their true paternity.

There is nothing singular in the fact, that many persons should have become satisfied of the truth of Gall's physiology by merely reflecting upon its accordance with sound mental philosophy; the same kind of thing happens, more or less, in every department of knowledge. Secondary proofs—those based chiefly on considerations of analogy—are appreciated the most easily, and, on this account, naturally weigh more with the great bulk of inquirers than would a chain of direct evidence, the tracing of which may involve an amount of labour and a degree of thought which are not always practicable. With respect to disputed questions, however,—those regarding which no universal consent obtains,—conviction of their truth, resting only on secondary evidence, is but rarely to be depended upon, because this latter, of itself, is never conclusive. In the case of Phrenology, the evidence of direct facts gained by personal observation can alone lead to a decision that is fixed and sure. When this shall have been obtained in such a manner, corroborative proofs will be felt by the student in double force; serving, however, it must once more be said, for illustration and elucidation of the several propositions in teaching the willing neophyte, rather than

for materials applicable in the confutation of some sceptical opponent, who may insist upon affixing to them some other interpretation, the fallacy of which cannot always be rendered apparent, unless by changing the mode of argument, and recurring to the primary proofs.

CHAPTER VIII.

HARMONY OF GALL'S PHYSIOLOGY OF THE ENCEPHALON, AND THE RECORDED EFFECTS OF ITS MUTILATIONS.

It has been shown in an early chapter how unsatisfactory and inconclusive have been the results attained, in all attempts to determine the particular functions of separate parts of the encephalon by mutilating the brains of living animals. It will have been seen that this procedure is utterly inadequate, as the primary step, to the discovery either of the cerebral or of the nervous functions. It has been conceded, however, that vivisections, when justly interpreted, are yet fitted for the corroboration of inferences already obtained by a sounder method of investigation, and also for shedding further light on facts that are previously understood in their principal relations. These views the author will now proceed to illustrate: and, in the first place, he proposes to exhibit the fallacy of certain conclusions deduced by encephalic vivisectors; secondly, he will demonstrate the complete harmony which subsists between the recorded effects of mutilations of the encephalon, and Gall's doctrine concerning its physiology; and then he will adduce instances of such lesions, authenticated in the most unexceptionable manner, which afford most valuable testimony of a subsidiary kind, eminently in favour of the identical deductions in opposition to which this class of facts is so often cited.

Before advancing to the execution of this design, it may be well to determine the general character of the phenomena which ensue on mutilations of structure, and the

influence which these latter exercise upon function; and, for this purpose, let us take the instance of the nervous system at large, many of whose offices are well understood. and which, in its general physiological relations, presents the closest analogy to the case of the encephalon. Let us ascertain the extent to which aberration in functional manifestation occurs in injury of the nerves, as also the mode in which actions of a sympathetic character are developed, and then we shall be in a better condition for reasoning upon the recorded effects of encephalic mutilations.

It is certain, that if some particular nerve sustain any injury, symptoms will very often be developed which indicate that irregularity in the performance of its function has been induced; this result may also occur if the centric origin of nerves be irritated. Now, although it is most probable that, whenever any division or portion of the nervous system is abnormally acted upon, some change in its functional operation takes place, this is not always appreciable from any external manifestation; and even when it is so, there is nothing like uniformity in different cases. Moreover, disturbances of function will coincidently arise in other structures which have sustained no direct lesion, on account of conditions of sympathy existing. dependent, as previously set forth, upon fibrous communication, contiguity of position, and association in the respective offices. All these things taken together, render it very difficult to settle the exact relations of injury of a nerve to the changes in functional manifestation that follow, because the conditions are constantly varying with the special circumstances of individual instances, and the variation in question is not of a nature always to be understood. Let us take, for an illustration, the example of a wounded finger, the nervous fibrils of which shall have been cut; sometimes, the only effect produced is pain, which entirely subsides on cessation of the inflammation; at other times

spasmodic twitchings of the contiguous fingers occur; and again, in other cases, tetanus even will be induced, exhibiting the remarkable extent of the sympathies which pervade the whole of the nervous system; and facts like these exemplify such as are explicable by direct fibrous communication. Then, again, the vicinity of parts as a source of sympathetic influence, may be exemplified by appeal to Magendie's operation upon the fifth pair of nerves, which was followed, if his account is to be relied upon, by instantaneous loss of sight. Lesions of any kind affecting the uterine nerves will notoriously provoke the sensibility of the mammary gland, a species of sympathy explained by reference to the allied character of their respective offices. In all these instances, which display the sympathies in question in the absence of any decided nervous communication at the periphery, it is likely enough that some anatomical connexion exists at the centres, of a kind scarcely to be traced by anatomical research, yet such a mere probability cannot of course be rendered the basis of any sound physiological theory, however reasonable it may appear to be.

If these illustrations of nervous sympathy be fairly representative of its general phenomena, we must keep such facts constantly in mind in our estimate of the value to be attached to mutilations of the encephalon, unless it can be shown that, in the latter case, there are reasons for inferring the existence of conditions of sympathy differing from those which obtain in the case of the nervous system,—a line of argument not very likely to be adopted. Let us, then, proceed to inquire, *first*, what the conclusions are which Gall's opponents have deduced from mutilations in opposition to his doctrine; and, *secondly*, to what extent they may be just, or, on the other hand, susceptible of disproof; then, by application of the principles which have been illustrated above in their relation to the nerves, let us see if certain facts gained by vivisection, and employed very

generally in opposition to phrenology, will not admit of a true physiological explanation, in correspondence and harmony with all that Gall discovered respecting the functions of the encephalon.

In proceeding with this division of the subject, the author shall confine himself, in the detail, to examination of the vivisections of the *cerebellum*, because, while those of the *cerebrum* have mostly been abandoned as inconclusive, some of the most able of modern physiologists continue to attach force to the mutilations which have been practised upon the former, and to advocate a doctrine founded upon them, —which doctrine they seek to maintain and corroborate in all sorts of ways, especially by comparative anatomy and pathological phenomena.

The present writer is one of those who think with Georget, that the organic connexion of the sexual instinct with the cerebellum is probably that particular point of phrenological doctrine, in favour of which the largest amount of proof is obtained; and, on this account, he conceives it to be most important that the facts and the reasonings of the vivisectors should be thoroughly sifted, with a view to discover whether or not their deductions concerning the cerebellum rest upon any solid foundation;—whether, in a word, the observations of Gall and his disciples on the one hand, or those of Flourens and his followers on the other, possess the higher physiological value.

Here there need be no repetition of the accounts which have been afforded by Rolando, Flourens, Bouillaud, and others, regarding their experiments upon the cerebellum, nor any further exhibition of their contradictory character. All this has been sufficiently dwelt upon in a previous chapter. At present, the author proposes to examine the validity of the doctrine which sets forth that the office of the cerebellum is to balance the body, and so to co-ordinate the action of the muscles as to bring about consensual movements. This physiological view was primarily ob-

tained by encephalic mutilation, and the vivisections of Flourens continue to form its principal support: it is, moreover, the only conclusion of the kind which has maintained its ground for any length of time. The doctrine is accepted and taught by men like Dr Carpenter, Dr Todd, and Mr Bowman, in this country; and by several leading physiologists on the continent. It has also been partially adopted by some who have publicly vindicated the general physiology of the brain taught by Gall—by Bouillaud in France, by Dr Cowan in this country, and by Dr Brigham in America. Even Mr Combe has exhibited a disposition to concede, in some degree, the conclusion in question, by assuming it as probable that a small portion of the cerebellum may be in subservience to some locomotive function. For all these reasons, the author will be at some pains to demonstrate the imperfections of the inference in question; dwelling, however, in the present chapter, principally upon the fallacies involved in the vivisections. He will also return to the subject in succeeding chapters, and expect, ere the conclusion of the work, to have shown,—not that no portion of the cerebellum has absolutely any share in maintaining muscular equilibrium, but—that such a proposition rests upon no adequate evidence drawn from any department of knowlege; whilst the doctrine of Gall is supported by every kind of testimony, whatever be the source from which it is sought.

Let us inquire, first of all, how far there is reason for regarding the power of balancing the body, and of executing combined movements through muscular agency, as one that is primitive and special, so as to need a separate organ for its encephalic centre; let us see if the ascertained offices of the brain and nerves do not yield every necessary condition for muscular co-ordination and for balancing, in such a way as to pre-engage, so to speak, the assumed function. And if it shall be found to be so, it were surely unphilosophical to infer the existence of a large and im-

portant structure for the accomplishment of what is very well performed without it.

Now, in the execution of combined movements, and in maintaining station and equilibrium, what takes place? The primary action is certainly cerebral; the will operates on the nerves, and through these upon the muscles. We *will* to perform a certain evolution, the *volitional* mandate goes forth, and the *combined action* of particular muscles ensues. Where does the agency of the cerebellum come in? Not certainly in volition, although some physiologists have thought that if the vivisections of Flourens prove any thing, it is "to show that the cerebrum is the more immediate seat of perception, and the cerebellum of volition."[1] A proposition of this kind, however, is too outrageous to require any serious comment. It is yet very important to know whether, in the estimation of physiologists who follow Flourens in this matter, the supposed balancing function has any dependence on consciousness; because if it be excluded, it is very difficult to form an idea of the power in question; and yet Dr Carpenter says, that "the cerebellum is not in any way the instrument of psychical operations."[2] Does the influence of this organ, then, come into operation *after* the cerebral volitional command has issued, and *before* it arrives at the muscles? There is no anatomical evidence whatsoever of such a thing. On the contrary, a particular disposition of many of the nervous trunks in *plexuses* seems to supply every intermediate requirement for the performance of movements that are consensual. In discussing the subject of nervous plexuses, Dr Carpenter observes as follows:—

"It is not unlikely also that, by this arrangement, a *consentaneousness* of action is in some degree favoured, as is supposed by Sir C. Bell; for comparative anatomy shows

[1] Dr Bostock. Physiology, vol. i., note, p. 276.

[2] Human Physiology, p. 217.

that something resembling it may be traced wherever a similar purpose has to be attained. Thus in the Hymenoptera, there is a similar interlacement between the nerves of the anterior and posterior pairs of wings, which act very powerfully together; whilst in the Coleoptera, in which the anterior wings are converted into elytra, and are motionless during flight, the nerves supplying each pair run their course distinctly. In the Octopus, or Poulp, again, the trunks which radiate from the cephalic mass to the eight large arms surrounding the head, are connected by a circular band, forming a kind of plexus, which evidently contributes to the very powerful and harmonious movements of the arms of this Cephalopod."[1]

In the successive steps of this supposed functional manifestation, we come at last to the muscles themselves. Are we to infer that the cerebellum furnishes these structures with some vital endowment, necessary to harmony in the voluntary movements, and in balancing the body? But if so, upon what evidence? Every conceivable condition required for the due maintenance of tension and antagonism in the muscles, is afforded by their own contractility, and by the excito-motory system of nerves; and, so far as balancing and harmony of movement can be supposed to have place without volition, the true spinal cord should be regarded as the actual nervous centre. It is well known that, in many of the inferior creatures, regular locomotive actions take place, after removal of the encephalon altogether, and this *must* be referable to the reflex agency of the spinal cord. "In the Dysticus," says Dr Carpenter,[1] "whose head had been removed, the stimulus of the contact of water immediately excited regular and continued locomotive actions, which lasted for some time." Further, it is observed by the same physiologist, "In the healthy condition of the human system, when the will is controlling

[1] Human Physiology, p. 89.

all the movements which are not immediately concerned in the maintenance and regulation of the organic functions, no such actions can be excited; but, in proportion as its control is lost, does the independent power of the spinal cord manifest itself."

Under all these circumstances, there appear to be no grounds whatever for regarding the balancing faculty as one that is special and distinct. To speak of harmony and co-ordination of muscular action, suggests *will*, or cerebral action; to refer to the complicated character of certain voluntary movements, directs our attention to the disposition of the nerves in plexuses; and when we revert to balance and movement in the muscles, in the absence of all consciousness, we are reminded of the true spinal cord. What is the part left, it may finally be asked, for the cerebellum to perform? It is high time that the disciples of Flourens should reply to this question definitively.[1]

It would yet appear to be certain, that mutilations of the cerebellum do really disturb the harmony of combined muscular action, and throw animals upon whom they are practised, very much off their balance; but why, for this reason, should we conclude the structure itself to constitute the general balancer of the body? Do the experiments themselves suggest no other inference? Are the results susceptible of no simpler explanation, than that of pronouncing an organ, like the cerebellum, to be connected with a function which is already engaged? Let us examine this matter somewhat in detail.

The general character of the sympathies which obtain in the case of the nerves, has been already dwelt upon; the conditions upon which they appear to depend have been illustrated; and it remains to be seen whether the several phenomena of convulsions, paralysis, staggering, whirling round, leaping, and so on, which have attended vivisections

[1] Human Physiology, p. 166.

of the cerebellum, do not allow of all reasonable explanation, through the anatomical and physiological relations which subsist between the organ in question and the medulla oblongata and motor division of the spinal cord.

It is an anatomical fact, that certain fibres from the anterior columns of the cord run into the cerebellum. As before stated, Mr Solly has the merit of being the first clearly to indicate this fact. "The corpora restiformia, or the processus e cerebello ad medullam oblongatam, are not, therefore, as they have usually been described, bodies which are formed solely by the *posterior* columns; nor are they bodies which consist of fibres from the posterior columns, to which some fibres from the anterior columns are added, the additional fibres lying perfectly parallel to those of the posterior columns; but they are bodies which consist of fibres that interlace in rather an intricate manner, the interlacing fibres consisting of some from the antero-lateral, and some from the posterior columns."[1]

Now, in this disposition of the upper extremity of the motiferous tract of the spinal cord, have we not ample data for explaining all the derangements that arise in muscular manifestation, on lesion of the cerebellum? Wherever there is close relation of two parts by interlacement of their respective nervous fibres, injury to one is well known to influence generally the actions of the other. When Legallois wounded the spinal cord in the dorsal region, perturbation of the heart's action ensued; but no one, from such a circumstance, would think of arguing, at the present day, that the office of the dorsal segment of the cord is to balance the heart; then, why insist upon the cerebellum balancing the body, because staggering, and other such symptoms, follow its mutilations? Moreover, the close propinquity of the medulla oblongata to the cerebellum, independently of direct com-

[1] Solly on the Brain, p. 158.

munication, supplies an additional reason for anticipating muscular disturbance on vivisections of the latter, seeing that these can scarcely fail to influence the corpora pyramidalia, and the continuous motor fibres, by *sympathy of contiguity*. But most of all, is it likely that, in the experiments now under discussion, there has been actual lesion of the medulla oblongata; or, at least, that it has been raised or displaced by the rudeness and violence of the operation? It appears to the author, that differences in the degree of dexterity and skill with which the mutilations have been practised, will, moreover, explain, in great measure, the diversity in result, which has characterised the vivisections, as related.

It has been maintained that the objection to the experiments of Flourens and others, on the ground of nervous sympathy, is untenable, for reasons stated, in the following terms, by Dr Carpenter:—"The fallacy of this objection, however, is shown by the fact, that the much more severe operation of removing the hemispheres, does not occasion such an aberration; the power of performing the associated movements, and of maintaining equilibrium, being remarkably preserved after the loss of them."[1] But then it should be recollected, that here there is no question of *severity of operation*, but of *conditions of sympathy*; in the case of the cerebral hemispheres, the mutilation does not occur, as in the instance of the cerebellum, contiguously to the medulla oblongata; and the maintenance of harmony in the movements after their removal, may be explained by the reflex agency of the spinal cord,—just as Dr Carpenter suggests to us the idea that locomotion, even in the human subject, may, in certain states of the mind, go on mainly through this influence.

When we carefully analyse the report made by Cuvier on the proceedings of Flourens, it becomes very obvious

[1] Human Physiology, p. 215.

that the varying effects of encephâlic vivisections upon the motor functions, are best explained by the greater or less degree of contiguity of the mutilated structures to the medulla oblongata. From the document in question, we learn that, after the removal of the *cerebrum*, the animal so mutilated becomes drowsy, exhibits no signs of will, makes no spontaneous motion; but that, when struck or irritated, it moves. Equilibrium is preserved in whatsoever way the creature is placed. If laid on its back, it rises; it walks if pushed. When it is a frog, it jumps if touched; when a bird, it flies if thrown into the air; it struggles, if annoyed; if water be poured into its throat, it is swallowed. The animal at times moves off without any apparent object in view, and stumbles repeatedly against the same obstacles. What do all these circumstances manifest? Why, so far as they go, results most remarkably harmonious with Gall's physiology of the cerebral hemispheres, wherein volition and intelligence reside,—psychical conditions obviously absent on removal of the cerebrum; the animal under such circumstances, according to the expressions occurring in the report, existing as if in a dormant state; the *movements*, which always needed to be *excited*, were clearly referable to the reflex function of the uninjured cord. When M. Flourens wounded the tubercula quadrigemina, a whirling round ensued, plainly owing to their communication with the motor portion of the nervous centres. "After all," says Cuvier's report, "it must be observed, that in too deeply extirpating these tubercles, we interfere with the medulla oblongata; and then violent convulsions, which last long, make their appearance." Is it not strange that the same reasoning should not have been made to explain the convulsions, the whirling round, and the staggering, which arose on cutting and scooping the cerebellum? for, let it be observed, in the case of the pigeon subjected to experiment, the removal of the first fragments of cerebellum did but produce weakness in the action of the muscles, and some diminution in their

harmony; and how should it have been otherwise? On attaining the middle of the cerebellum, and so coming nearer to the medulla oblongata, the creature displayed universal agitation; and, on removal of the whole, it no longer had the power of walking or flying; and, placed upon its back, it was unable to raise itself. And where is the wonder, if the anatomical connexions of the organ be but considered for a moment?

But, indeed, we possess a species of evidence in absolute and incontrovertible disproof of the doctrine of Flourens respecting the cerebellum, to which we may here advert—evidence which no sort of reasoning from any other class of facts can invalidate. This consists in the retention of the assumed function after complete removal of the organ. "I have seen," says Magendie, as quoted in a previous chapter, "and have demonstrated to others, a great many times, in my course of lectures, animals deprived of cerebellum, and which nevertheless executed very regular movements." And, as before stated, a pigeon whose cerebellum had been destroyed by M. Fodera, walked backwards and forwards; and to do this, some co-ordination of muscular action was required. If these statements be doubted, so may those of M. Flourens: the authority in the latter case is no better than in the former. Indeed, some might contend that Magendie, at any rate, from great practice, should be regarded as the most expert of vivisectors. It might further be said that, in this physiologist's experienced procedure, the medulla oblongata was in all probability but little interfered with, and that this circumstance may explain the persistence of harmonious movements after the operation.

But, in point of fact, it is difficult to conceive the occurrence of any severe mutilation of the nervous system, involving a lesion of sensibility, without the production of some such effects as paralysis, convulsions, staggering, or whirling round, by excitation of the appropriate ner-

vous centres. . It is certain, at any rate, that injuries inflicted near the base of the brain will constantly exert some disturbing influence on the muscular system; and how, in an animal, can this be evinced except by some loss of harmony in the movements and power of co-ordinating the locomotive structures? M. Flourens himself, in one of his papers,[1] recounts certain results of cutting the terminal branches of the auditory nerve as they ramify in the semicircular canals, which seem to vary very little from those obtained by some vivisectors in their doings with the cerebellum. If the horizontal canal on each side was divided, horizontal movements of the head took place from side to side, and rotation of the whole body. Division of the inferior vertical canals on each side produced vertical movements of the head, but the animal lay forwards. If all the canals were divided, all sorts of violent motions took place. All which effects are obviously referable to sympathy, and to the reflex agency of the nervous centres. Yet who shall doubt, that if some young aspiring vivisector were to injure the cerebellum, and, in the process, were to witness the very symptoms recorded by Flourens as coming from lesion of the acoustic nerve—who shall doubt that the results would be appealed to as certain corroboration of the muscular functions of the organ? The hasty inferences, indeed, which have been deduced from vivisections regarding the office of the cerebellum, constitute an excellent specimen of that gratuitous kind of reasoning which is but too characteristic of the proceedings of a certain class of experimentalists, who, in their excessive zeal for mere facts and striking novelties, abandon almost entirely the just requirements of a sound and philosophical induction.

There are really no arguments whatsoever of the slightest weight, that prove the power of co-ordinating

[1] Memoir de l'Academie des Sciences, t. iv., p. 445 et seq.

muscular action to be an independent, primitive faculty; nor do the mutilations which have been practised upon living animals in any way determine the cerebellum to be the organ of such a function. And at the same time it may be asserted, further, that nothing which is related in the accounts furnished of the vivisections of this structure, is in the remotest degree opposed to the physiology of Gall. In none of the instances is there the slightest evidence adduced, whereby it is so much as pretended that the sexual instinct survived the loss of the organ; so that the harmony of this class of facts with the phrenological doctrine is, at least, *not negatived.* But then it will be said, that as no influence was exerted upon the generative function by these powerful actions upon its assumed encephalic centre, neither was such harmony *affirmed.* Yet, where is the proof that the sexual instinct was actually unaffected by vivisections of the cerebellum? When extraordinary violence is done to any structure, the effect is very often to paralyse its influence altogether: what reason is there for supposing that this may not have been the case in the instances in question? Moderate violence, inducing what may not much exceed *stimulation*, very generally provokes functional manifestation; but, then, this merely stimulant action is certainly not what is brought about in severe mutilations, and in the destruction of an organ. Again, it should be remembered that the habitual state of the function now under consideration is one of quiescence, and, on that account, it is one not so likely to reveal the fact, even when present, of its loss or diminution. Further, in such creatures as the pigeon and others, in which these lesions of the cerebellum have been practised, the outward indications of internal activity of the instinct (did we even suppose this to exist) could not very well be appreciated under the circumstances in question. But, indeed, how entirely vain it must be to expect, for a moment, that any animal distracted with pain could evince feelings which every body

knows demand psychical concentration, in an eminent degree, for their sensible display. In a few words, it may be said of these vivisections of the cerebellum, that whilst they most certainly make nothing *against* the physiology of Gall, they have not been calculated, in their very nature, to yield any thing of moment in its favour.

Many mutilations, however, are on record, which most unmistakeably corroborate Gall's doctrine concerning the cerebellum; some of these are recounted by Gall himself, as having come under his own observation; there are others related by Vimont, as having occurred within his particular experience; and some very important cases, ranking in a like category, are detailed by Baron Larrey, in his *Memoires de Chirurgie et Campagnes*. Other writers have also accumulated instances tending to a similar result,—a result harmonious with phrenology, and supplying secondary evidence of its truth.

Before proceeding to exemplify what has just been stated by any narration of cases, it must here again be stated, that all such facts as those which the subjoined accounts include, are utterly inadequate to *prove* any physiological proposition; they are cited in the present place, because they *confirm* a truth which has already been established by a more direct, legitimate process; they furnish excellent subsidiary testimony; the phenomena receive their simplest and most obvious explanation, interpreted by the doctrine of Gall; for the statements will exhibit a reciprocal influence, maintained between the cerebellum and the external organs of generation, which is only intelligible on the theory of *association in function;* they admit of no elucidation by attention to other conditions of sympathy, for, unlike the facts of Flourens and other advocates of the muscular office of the cerebellum, neither immediate communication by nervous fibres, nor contiguity of position, will afford reasons for the action of one upon the other, which is affirmed to have taken place.

Gall relates that, at Vienna, he was consulted by two officers who had become impotent in consequence of blows from fire-arms, which had grazed the napes of their necks. At Berlin, Dr Formey spoke to him of a man who, in consequence of a wound in the region of the cerebellum, had, at first, high and irregular excitement of the correspondent instinct, and then fell into the very opposite state; a recovery, in all respects, took place in six months from the period at which the injury was inflicted. The case was interpreted pathologically, thus,—the inflammation which ensued upon the wound was considered to have induced irritation of the cerebellum; this was followed by debility, as an ordinary consequence of undue excitation; and. finally, restoration to ordinary health took place, as is not unusual, from the mere lapse of time.

The following cases are reported by Baron Larrey, in the work just mentioned; they are, here, slightly abbreviated from a translation given by Mr Combe, in an appendix to his English version of Gall on the Cerebellum. Greater confidence may probably be placed in these recitals, and they may carry more weight, as Larrey had no phrenological prepossessions, or reputation, of which he could be supposed to be jealous.

François (Auguste) Marechal des logis, of the Horse Guard Artillery, received, at the battle of Benevento, a wound from a musket-ball, which traversed from side to side the insertions of the extensor muscles of the head, *grazing upon the two inferior occipital swellings* (corresponding to the lobes of the cerebellum), which, being very prominent in this individual, were denuded or stripped of their tendinous attachments. Larrey dilated the two openings made by the ball, and extracted a portion of his shirt which had remained in the wound, and then dressed it with emollients. A variety of symptoms ensued, the most remarkable of which, however, were a *diminution in the size of the testicles, which fell into a state of atrophy or wasting.*

The penis was also reduced in size, and remained without action: however, the wound healed, and all the symptoms disappeared in about two months.

Bigot (Réné), chasseur à cheval, or light horseman, of a strong and very amorous constitution, received, at the same battle, a cut from a sword, which divided the skin and all the convex or projecting portion of the occipital bone through to the dura mater, of which a very small part was touched. The right lobe of the cerebellum was seen through the opening of the dura mater. The slightest pressure upon this organ caused giddiness, fainting, and convulsive movements.[1] From the first day, the patient lost the sight and hearing of the right side. He experienced at the same time acute pain in the course of the dorsal spine, and a kind of tingling in the testes; these diminished sensibly, and in fifteen days were reduced to the size of a bean. *Soon after he lost all idea or recollection of past sexual excitement.* The patient at one time gave hopes of cure, except with regard to the functions of sight, hearing, and generation; but, soon relapsing, he died ultimately of tetanus, on the thirty-ninth day, being the 7th February 1807. On dissection, great loss of substance at the occiput was found; the opening of the dura mater corresponding to the right lobe of the cerebellum which was shrunk, was of a yellow colour, without suppuration or effusion. The medulla oblongata and upper part of the spinal cord were of dull white, of firmer consistence, and reduced in size by one-fourth. The nerves arising from these parts were likewise wasted.

Pierre Soult, 22d chasseurs, received from a Mameluke, at the battle of Salehyeh, a cut from a sabre, which, after dividing the skin and *external protuberance of the occipital bone*, divided the extensor muscles of the head as far down as the sixth vertebra. The man was cured; but Larrey

[1] This circumstance strikingly illustrates the extensive sympathies determined by mechanical violence done to the cerebellum.

adds in a note,—"I have since had occasion to see this soldier, who declares that he has been deprived of his generative powers ever since that wound."

Now, the above cases are not absolutely inexplicable on some other hypothesis than that of the cerebellum being the encephalic seat of the sexual instinct, because, the lesions taking place in such instances being of a nature seriously to impress the whole cerebro-spinal axis, it may be assumed that, under such circumstances, the spermatic nerves so shared in the general injury as to have led to deterioration in the external organs. But, although this might constitute a *possible* explanation of the facts, most assuredly it would not form the *probable* one; much more likely is it that *association in function* originated the sympathetic disturbance, in the same way as the mammary gland will sometimes take on irregular action sympathetically with the uterus. There are very few problems affecting morbid agency which do not *admit* of several possible modes of solution; it is meet and just, however, to adopt the one which harmonises the most readily with our experience of that which obtains in analogous matters.

In this view of things, and with such reserves as the above reasonings imply, the author will now adduce another class of mutilations which tend, yet more decisively, to corroborate the physiology of Gall, in its relation to the office of the cerebellum; allusion is made to facts which exhibit degeneracy of this structure, consequent upon removal of the testes.

Gall caused several rabbits to be castrated, some on the right side and others on the left. Having had them killed six or eight months afterwards, he found in all, without exception, that the lobe of the cerebellum of the side opposite to that on which the castration had taken place was smaller, and that the occipital swelling was flatter than on the other.

These experiments have been repeated by Vimont, with varying results. In a memoir which he presented to the

French Institute, in 1827, he stated that he had observed no diminution in either lobe of the cerebellum of four rabbits, which he had castrated on one side, and preserved alive during eight months. Subsequently, however, he operated on four other rabbits, which he fed during *eighteen months*, and after death he noticed a very perceptible diminution in the opposite lobe of the cerebellum.

" Baron Larrey," says Gall, " sent to me a soldier who, in undergoing an operation for hernia, had lost the right testicle. Several years afterwards, his right eye became weak. He began to squint with the diseased eye, and could scarcely any longer distinguish objects with this eye. I examined the nape of his neck, in presence of the two physicians who had brought him, and I found the occipital swelling of the left side much less prominent than that of the right side. The difference was so perceptible, that the two physicians were struck with it at first sight."

M. Dannecy communicated to Gall the following fact, which he had observed himself in the hospital of the school of medicine, in presence of M. Patrix, assistant surgeon of the establishment, and of several pupils. It is recorded under the No. 108 (15th July 1817,) in the volume of the pathological observations made in the hospital. In the dissection of Jean Michael Brigaud, who died the 14th July, 1817, after having, on the 30th December, 1815, undergone an operation for diseased testicle of the right side, the following remarks were made:—" The brain and the cerebellum were covered with a light layer of a white transparent albuminous substance. The left lobe of the cerebellum was much softer and flatter than the right lobe. The convolutions or folds appeared also more sunk or obliterated on the same side. Each of these lobes having been opened at exactly six lines of distance from the lateral portion, corresponding to the medulla oblongata, we were surprised to see how much greater the proportion of the white matter and of the grey matter was in the right lobe.

The difference was estimated at more than a third. The interior development of the cranium also corresponded to this difference."

Many other such cases might be cited. The above, however, sufficiently serve for the present purpose, which, according to the terms in which this chapter is headed, is simply to illustrate the harmony and correspondence which subsist between the physiology of Gall, and the recorded effects of mutilations. And surely the experiments performed by Rolando, and Flourens, and Bouillaud, possess nothing like the force, in relation to the conclusions which their authors have drawn, that such instances as those just recounted have, in regard to the inferences which phrenologists deduce concerning the cerebellum. It is true that they do not *prove* the point, but they most powerfully *confirm* it; and, of all the subsidiary testimony capable of being cited in support of any physiological proposition, it is impossible to conceive of any that is more decisive than that which consists in a demonstration of the degenerative effects produced by unilateral castration on a single lobe of the cerebellum.

Still the inquiry may arise, Does the degeneration take place with such regularity and uniformity, as to authorise its being regarded in any other light than that of an accidental circumstance? May it not be, that the facts recorded have been *selected* from amongst many others, where no such effects have been witnessed? And if this be so, is not their value, as bearing upon any positive inference, very seriously diminished? The objections which these interrogations imply, are more plausible than valid; because, in point of fact, the abnormal actions of the living organism do but rarely exhibit an invariability in sequence. There is scarcely a single lesion that can be inflicted upon the body, which provokes universally a given train of symptoms. Let us recur to the analogous case of the uterus and the mamma. Here we very generally find, that sympa-

thetic phenomena develope themselves whenever an unwonted influence is exerted upon either structure. Yet this is far from being a circumstance that is always to be detected. We will take a very simple illustration—that which pregnancy affords. In a large number of instances, the breast, during this state, becomes larger, fuller, and more sensitive, frequently even displaying the presence of milk, in very considerable amount; but then cases occur in which little or nothing of the kind is appreciable. On account of the exceptions, however, no one thinks of doubting the intimate relation which subsists between the womb and the breast; and a unity of purpose in the respective functions of these structures (both being concerned in the supply of nourishment to the early being), is not the less recognised. The reciprocal influences, when observed, yield corroboration and illustration of the leading truth, primarily ascertained by direct process of demonstration; and because every case alike does not exhibit the mutual sympathies which *commonly* have place, the instances wherein the fact is so, are not put down to the account of what is fortuitous and accidental, but receive an interpretation from what is already known of the functions in question, furnishing examples in confirmation of a great physiological law—that which proclaims that there is a certain consentaneousness of action in different organs where, in function, there is something like unity of purpose.

The relations between the cerebellum and the exterior apparatus should be estimated and regarded in a like point of view; if the vital conditions of either structure display obvious sympathy with those of the other, the phenomena should receive explanation from the knowledge already obtained, by direct evidence, concerning the functions of these organs. If removal of the testes be at times followed by atrophy or degeneracy of the cerebellum; and, reciprocally, if lesion of the latter structure be also succeeded by occasional derangement of the external organs;

a remarkable fact is exhibited, in illustration of the mutual sympathy which obtains between structures associated in function,—a fact which strengthens the convictions of those who have already satisfied themselves, by direct observation, of the justice of Gall's physiology of the cerebellum, and one which is calculated to furnish the sceptic with testimony of a striking character, though but of a secondary kind. As before maintained, the circumstance of the sympathetic phenomena not being always developed, proves nothing; similar exceptions obtain in all analogous instances.

There has been an attempt made by M. Leuret, to disprove the sexual functions of the cerebellum, by a series of statements intended to antagonise the class of facts just adduced; their value, however, as related to the essential proposition, is entirely misunderstood, when cited in disproof of Gall; and it is clear, from the account to which they have been turned by opponents, that no correct appreciation of the evidence upon which phrenologists rest their conclusions is formed by many otherwise able physiologists. Allusion is here made to certain recorded weights of the brain and cerebellum in the case of mares, geldings, and horses unmutilated, furnishing results entirely opposed to such facts as had been observed by Gall, Vimont, and others; results, moreover, which, in the estimation of some parties, are calculated to destroy altogether the phrenological doctrine in so far as the cerebellum is in question. The statements of M. Leuret shall receive a detailed examination; because, if passed by, or allowed to remain uncontroverted, they are likely to exert a most undue influence upon the minds of parties little accustomed to estimate the relative value of different kinds of evidence. The annexed account is taken from Dr Carpenter's Human Physiology;[1] and the facts recorded appear to have great weight with this able writer:—

[1] Second Edition, pp. 220-22.

"The following is the result of a series of observations on this subject, suggested by M. Leuret,[1] and carried into effect by M. Lassaigne:—The *weight* of the cerebellum, both absolutely, and as compared with that of the cerebrum, was adopted as the standard of comparison. This was ascertained in ten stallions, of the ages of from nine to seventeen years; in twelve mares, aged from seven to sixteen years; and in twenty-one geldings, aged from seven to seventeen years. The average weight of the cerebrum in the stallions was 433 grammes; the greatest being 485 gr., and the least (which was in a horse of ten years old) being 350. The average weight of the cerebellum was 61 gr.; the greatest being 65 gr., and the least 56 gr. The average proportion borne by the weight of the cerebellum to that of the cerebrum, was, therefore, 1 to 7·07; the highest (resulting from a very small cerebrum) being 1 to 6·25; and the lowest (resulting from an unusually large cerebrum) being 1 to 7·46. Throughout it might be observed, that the variation in the size of the cerebellum was much less than in that of the cerebrum. In the twelve mares, the average weight of the cerebrum was 402 gr.; the highest being 432 gr., and the lowest 336 gr. That of the cerebellum was 61 gr.; the highest being 66 gr. (which was in the individual with the smallest cerebrum), and the lowest 58 gr. The average proportion of the weight of the cerebellum to that of the cerebrum was 1 to 6·59; the highest being 1 to 5·09, and the lowest 1 to 7. The proportion was, therefore, considerably higher in the perfect female, than in the perfect male. In the twenty-one geldings, the average weight of the cerebrum was 419 gr.; the highest being 566 gr., and the lowest 346 gr. The average of the cerebellum was 70 gr.; the highest being 76 gr., and the lowest 64 gr. The average proportion was, therefore, 1 to 5·97; the highest being 1 to 5·16, and the lowest 1 to 7·44. It is curious that this

[1] Anat. Comp. du Systeme Nerveux, tom. i. p. 427.

last was in the individual which had the largest cerebellum of the whole; but the proportional weight of the cerebrum was still greater.

"Bringing together the results of these observations, they are found to be quite opposed to the statement of Gall. The weight of the cerebrum, reckoning the cerebellum as 1, is thus expressed in each of the foregoing descriptions of animals:—

	Average.	Highest.	Lowest.
Stallions, . .	7·07	7·46	6·25
Mares, . .	6·59	7·00	5·09
Geldings, . .	5·97	7·44	5·16

The average *proportional* size of the cerebellum in geldings therefore is so far from being *less* than that which it bears in entire horses and mares, that it is positively greater; and this depends not only on diminution in the relative size of the cerebrum, but on its own larger dimension, as the following comparison of *absolute* weights will show:—

	Average.	Highest.	Lowest.
Stallions, . .	61	65	56
Mares, . .	61	66	58
Geldings, . .	70	76	64

The difference is so remarkable, and appears, from examination of the individual results, to be so constant, that it cannot be attributed to any accidental circumstance, arising out of the small number of animals experimented on. The average weight of the cerebellum in the ten stallions and twelve mares is seen to be the same; and the extremes differ but little in the two; whilst the average in the gelding is more than one-seventh higher, and the *lowest* is considerably above the *average* of the preceding, while the highest far exceeds the highest amongst the entire horses. It is curious that Gall would have been much nearer the truth, if he had said that the dimensions of the *cerebrum* are usually reduced by castration; for it appears from the following table that this is really the case:—

	Average.	Greatest.	Least.
Stallions, . .	433	485	350
Mares, . .	402	432	336
Geldings, . .	419	566	346

The weight of the largest cerebrum of the gelding is far above the highest of the stallions; but it seems to be an extraordinary case, as in no other was the weight above 490 gr. If this one be excluded, the *average* will be reduced still further, being then about 412; this may be seen, by looking over the whole table, to give a very fair idea of the usual weight in these animals, which is therefore *less*, by one-twentieth, than the average of the stallions."

These facts then, if facts they be, do really seem, on a superficial consideration, not only to disprove the accuracy of observation, on the part of Gall and some of his followers, in regard to the influence of mutilations, and to the relative development of structure in the sexes, but even to invalidate, probably, the best established proposition in phrenology; and, in such a way, to discredit very materially the entire doctrine. A close attention to this matter, however, will exhibit several sources of fallacy in these vaunted records (even on the assumption of complete accuracy in their minutest particulars), calculated to show their inconclusiveness, to say the very least.

For, it is quite certain that forty-three instances—ten stallions, twelves mares, and twenty-one geldings—are utterly insufficient to exemplify an average state of things, —an assertion which no one will doubt who has directed the slightest attention to vital statistics; the fact, however, shall be rendered obvious enough presently, by exhibition of contrariety of results in this species of investigation, when much larger numbers have been taken. Again, there is nothing to prove that the cases were taken indiscriminately; they *may*, in some measure, have been selected; and it has never been maintained by phrenologists that all males, of any species, have the cerebellum more largely developed

than the females, but only that this is the case in a majority of instances. The true test of Gall's doctrine would have consisted, in comparing the volume of the organ with the intensity of function, as manifested in the particular examples, and not in contrasting the weight of a small number of male cerebella with that of those belonging to a few females. M. Leuret adduces no evidence to show that the sexual instinct was not really more powerful in the mares in question, than in the horses. If we take the human kind, nothing would be more easy than to procure twelve females and ten males, and to ascertain, as a highly probable fact at least, that the amative passion was more intense in the former than in the latter; but then the phrenologist, under such circumstances, would expect the development of cerebellum to correspond. Then, with regard to the geldings, in which the heavy cerebella were detected, we are not informed at what age the mutilations took place. Had not the cerebellum, at the period of emasculation, attained its maximum development?—in which case no diminutive size was confidently to have been anticipated. The decay of the cerebellum, in consequence of mutilation, was not stated by Gall to be a constant fact: and, as will have been seen in a previous page, Vimont relates instances in which the occurrence did, and in which it did not, take place. Spurzheim merely asserts that the further growth of the organ is checked, if emasculation be performed very early; he writes: "Sic castratio cerebelli incrementum imminuit; nam in hominibus et animalibus castratis cerebellum crescere desinit. Quamobrem eunuchis atque animalibus, in prima ætate castratis, est collum valde exiguum, et copulandi cupido nulla. Contra, homines cæteraque animalia, post plenos annos castrati, quamquam testium expertes, sensum tamen eroticum et copulandi stimulum conservant."[1]

[1] Physiognomical System, 1815. Note p. 280.

In the case of the geldings, however, is it quite certain that no atrophy of the cerebellum had occurred? Relatively heavy as the structure revealed itself to be, had not its dimensions been yet more considerable prior, than subsequent, to the operation? These are important points to be determined, before we should attach any decisive value to the statements of M. Leuret; and they are fair and reasonable matters of inquiry, when we consider the paucity of instances which go to form the recorded average.

But there is yet another point of view in which these statements should be regarded. The observations of Gall and the phrenologists have reference always to the *volume*, not to the *weight*, of an organ. Undoubtedly, in cases where the organic quality is exactly similar, weight will maintain a uniform relation to volume; but disease—any departure from the normal condition of things—may so alter the intimate composition of the encephalic substance, as probably to influence very sensibly the correspondences of weight and volume. M. Parchappe, in his researches on the encephalon, exhibits, in tables, a variety of facts which appear to show this point, so far at least as statistics on a moderate scale can determine any such matter.

The following table, taken as an example of many others occurring in the work just mentioned, gives the mean weight of the cerebral substance in ninety-four instances; the facts occurred within the experience of M. Cruveilhier; they exhibit the differences induced, seemingly, by insanity, the observations and comparisons having been made in the case of twenty-nine sound-minded men and twenty-five who were mad, and of eighteen sound-minded women and of twenty-two who were mad:—

Mean weight	Of the Encephalon.	Of the Cerebrum.	Of the Cerebellum.
With 29 sound men,	1·323	1·155	179
25 *mad* men,	1·390	1·173	162
18 sound women,	1·210	1·055	147
22 *mad* women,	1·262	1·095	161

The present writer is not one who places any very great degree of value upon mere statistics, when the object is to gain demonstration of some doubtful matter affecting such questions as the one now under discussion; because an uncertainty always remains whether the general circumstances of the aggregate of facts have been so nearly equal, as to permit decisive weight to be attached to the particular difference of result, concerning which the inquiry is made. And, judging from what occurs in other analogous investigations, doubt must ever prevail with respect to the numbers required to yield the just average, as it exists in nature.

The true value of statistics in these researches, the author believes to consist in their aptness for corroborating results procured by closer and more direct investigation, and in their tendency to suggest extended inquiry, or to cause revision of previous researches, so as to bring about more accurate results. Without, then, recognising any conclusiveness in such a table as the one exhibited above, the writer directs attention to the fact, that it indicates a tendency, in mania, to increase the *weight* of the encephalon, although most writers on the subject represent a diminution in its *bulk* to be a very frequent consequence; the table further goes to show that, with insane men, the cerebellum becomes lighter, and with women heavier, under the like circumstances. Now, if any great degree of importance could be attached to such results as these, it might be contended that Leuret's facts and Gall's physiology harmonise in the following manner: After dismemberment, the cerebellum exists in an *abnormal* condition, as occurs in mania; this may occasion a deposition of matter, in the process of nutrition, specifically heavier than that which exists in the normal state, and so yield a structure capable of weighing more than before, though of reduced volume. But, in truth, such a procedure would very much resemble *special pleading;* the author is convinced that no valid conclusion

whatsoever can rest upon any such limited number of instances.

The table just cited contains also another element, calculated to show the fallacy of deducing results from so small an amount of facts; this circumstance may be illustrated thus: Suppose that it had been maintained, after acknowledgment of the fact that disease *modified* structural density, that the cerebellum had its weight increased always by madness, the author of such a proposition might support the idea by conclusions deduced from no less than fifty-four cases (as shown with respect to the men in the foregoing table), a number surpassing by eleven the number of Leuret's horses; then might arise some opponent to the notion, and with forty instances of women (as shown above) exhibit a state of things just the contrary! Yet both parties in such an affair would have been right in their facts, the number of which, however, would have been far too inconsiderable to warrant *any* general inference.

The above considerations must at least demonstrate *possible* sources of fallacy in Leuret's facts; nothing that has been advanced disproves them, but surely their value, as *determining* any conclusion, can no longer be insisted upon by the opponents of Gall. Any one who is disposed to confide, to any extent, in a slender array of figures in relation to such inquiries, will have seen that, judging from corresponding data, weight and volume of the encephalic structures do not vary coincidently, and that morbid states influence the one condition as well as the other, and this under no appreciable rule. Such questions as these, however, it should now be clear enough, must be settled by processes of a very different character.

But "it is necessary to be on one's guard against facts which are attested only by the adversaries of a science, because it is well known to what extent the spirit of speculation may lead to falsehood in assertion;"[1] and, after the

[1] Broussais. Cours de Phrénologie.

experience which phrenologists have had in this respect, the most illiberal opponent should extend to them a pardon when they doubt the accuracy of M. Leuret, even in the affair of fact. It is the present writer's conviction, that some mistake (not wilful it is to be hoped) has found its way into the proceeding by which the instances were examined and reported, because they are completely opposed, in their general character and tendency, to the recorded observations of many other able men in various countries. The author, judging from his own experience, considers that Gall's discovery, with regard to the cerebellum, is an affair of rigorous demonstration; and, for this reason, he must hesitate before he admits the truth of statements even in *seeming* opposition; for, once more, in the language of Bouillaud, "if every theory found in contradiction with a single fact well observed be false, so every fact found in contradiction with a theory rigorously demonstrated has been badly observed."

As it has been previously shewn, there is no *actual* contradiction to the results of Gall's observation in Leuret's statements; the limited amount of the cases readily permits them to be explained, so as to leave the phrenological doctrine unaffected; there is, nevertheless, *pro tanto*, a case made out against it, if the facts have been accurately observed, and the instances indiscriminately taken. On this account, it is very difficult to divest one's self of the impression, that, either through negligence or some other cause, they have been misapprehended or distorted. Weights and measurements have, on several prior occasions, been adduced in order to overthrow the physiology of Gall; and it is very remarkable that, when they have been procured by parties who had previously rendered themselves, like M. Leuret, conspicuous as opponents, the result has always been made to corroborate the foregone conclusion. This circumstance is of itself justly calculated to excite suspicion; more especially, as it may be noticed, that, when

corresponding instances and groups of figures are collected by individuals uncommitted upon either side, the result is very generally the other way. Mr Stone, Sir William Hamilton, and Professor Tiedemann have, at several times, rendered themselves prominent in their opposition to phrenology; their positions have been largely maintained by figures, representing weights and measurements; these shall be carefully examined. On the other hand, M. Parchappe, Dr Morton, and Dr John Reid, have weighed and measured to a very great extent, without any apparent object beyond that of accurately recording fair average results; and these latter have presented not even a *seeming* opposition to the observations of Gall and his disciples, whilst, in several particulars, they have supplied new and very important confirmation. When all these circumstances have been reviewed in detail, the reader will be in a somewhat better condition for estimating the worth of any inference, deduced from such statements as those of M. Leuret.

About sixteen or seventeen years ago, Mr Thomas Stone, at that time of Edinburgh, published the results of certain weighings and measurings of crania, which the prejudiced and the superficial then considered to have given to phrenology its permanent *quietus;* a pamphlet which he wrote "On the Phrenological Development of Burke, Hare, and other atrocious Murderers, &c.," was spoken of, in the following terms, by the *Edinburgh Literary Journal:*—

"This is one of the most efficient knock-down blows which phrenology has yet received. Nobody can read this pamphlet and believe in phrenology; we question whether Mr Combe himself can. We should not be surprised to hear of his abruptly terminating his lectures in Dublin, and going into retirement for the rest of his life. 'Assail our facts, and we are undone; phrenology admits of no exceptions,' has been his continual exclamation. 'Eh! bien,' says Mr Stone, 'we'll take a look at your facts, and see how they answer.' Mr Stone's former pamphlet on the

same subject was a learned and able one, but this is a thousand times more convincing, because there is no theorizing in it, nothing but plain statements and incontrovertible deductions. He has 'assailed their facts' with a vengeance, and has succeeded in making it perfectly clear that there is no such thing as a well-established *fact* in the whole science. We do not speak rashly, nor do we speak partially."

In this (for a time) celebrated pamphlet, Mr Stone exhibited tables, the object of which was to prove mathematically, that the heads of depraved murderers, like Burke and Hare, were very favourably developed; and this he assumed to do, by comparing them with the heads of twenty-eight Englishmen, twenty-seven Irishmen, and twenty-five Scotchmen,—in all, eighty cases. The comparisons were, professedly, based upon systems of measurement and weighing that had no correspondence with any rule, or method laid down, either by Gall, or by any of his followers; for, in the first place, the measurements had relation only (and this after an erroneous plan) to what phrenologists call the *length* of an organ; no attempt was made to estimate, or determine, the *breadth*. The facilities for misstatement under these circumstances, with only a *little* latitude of conscience, are very obvious to any one having the least practical acquaintance with the subject. Then, in measuring living crania, in order to ascertain their *capacity*, Mr Stone took the length, breadth, and height, and *added these together!* The humblest arithmetician must see the absurdity of such a proceeding, which, however applicable for the hatter's purpose, is most preposterous in reference to phrenology; besides its arithmetical absurdity, it takes no cognisance of the general elevation of the coronal region, or of the depth of the forehead, and, tested in such a way, the low wretched head of Hare, or of a Carib, might seem to be equal to that of some of our greatest men. Weighing and measuring were put in requisition, also, for

the purpose of making out the relative development of the individual organs; the plan consisted in filling crania with sand, in order to determine the weight of the brain; then, a comparison was made between this—the *weight*, and the *length* of particular organs, and this was called *the ratio between the size of an organ and the size of the head!* A glaring error, well known to mathematicians as the comparison of incommensurable quantities.

It is hardly worth while, at the present day, to extend the criticism upon these weights and measurements any further; it may be sufficient to state that, on the cessation of the first flush of triumph at their publication, they fell into absolute disregard, even with the opponents of Gall's physiology.[1] Absurd as these measurements are, so signally ignorant do some writers continue concerning the merits of Phrenology, that Mr Thomas Monck Mason, B.A., in a work published in opposition to the author of the "Vestiges of the Natural History of Creation," so lately as 1845,[2] actually founds his rejection of the science on the measurements by Mr Stone, reported at second-hand by the late

[1] Mr Combe published a reply to Mr Stone in the sixth volume of the Phrenological Journal; and the following extract from it exemplifies, in an amusing way, the accuracy of the anti-phrenological calculations:—

"The results of his measurements and tables show that he has proceeded on no intelligible or consistent principle whatever. They are so palpably ludicrous that they actually thrust a '*reductio ad absurdum*' upon the notice of the reader. For example,—in discussing Hare's head he says, "The proportion of *Destructiveness* to the size of the head is as 1 to 2·319. The proportion of Benevolence to ditto is as 1 to 2·555. The proportion of Conscientiousness to ditto is as 1 to 3;" that is to say, of Hare's head, Destructiveness constitutes within a small fraction of one half; it is as 1 to 2⅓; Benevolence constitutes very nearly another half, for it is as 1 to 2½; and Conscientiousness is exactly one-third; so that the size of these three organs exceeds that of the whole head which contains them, and all the other 32 organs have no size whatever!

"In citing this example I am not taking Mr Stone at disadvantage, catching him tripping, at it were, in some huge calculation amidst his

[2] "Creation by the Immediate Agency of God, as opposed to Creation by Natural Law," by Thomas Monck Mason. 1845.

Dr Milligan ! Nevertheless, it is certain that strong reasons afterwards arose for doubting something more important than the accuracy, or even than the judgment, of Mr Stone.

Prefixed to the "Anatomy of the Brain, by Dr Monro," published about fourteen years ago. there is an account of Experiments on the Weight and Relative Proportions of the Brain, Cerebellum, and Tuber Annulare in Man and Ani-

mighty chaos of decimals; on the contrary, his whole tables present similar absurdities to the eye. 'Table II.' will serve as an illustration.

"'Table II. Measurements of the Heads of living persons,—Englishmen.

"'Average size of the head in 28 Englishmen, . . .		13·557
"'Average proportion of *Destructiveness* to the size of the head in ditto, ...	2·282	
"'Ditto ditto of *Benevolence* to ditto in ditto, . .	2·464	
"'Ditto ditto of *Conscientiousness* to ditto in ditto, .	2·785	
"'Ditto ditto of *Acquisitiveness* to ditto in ditto, .	2·437	
"Proportion of these four organs to the size of the whole head, ...		9·968
"Remains for the other *thirty-one organs*, . .		3·589

"It is quite obvious that, if Mr Stone had proceeded two steps farther in his measurement of particular organs, he would again have made the sum of the separate dimensions of a few organs greater than the size of the whole head.

"He is equally unmerciful to his Scotchmen and Irishmen.

"The size of the whole head in the Scotchmen is . .	13·694
"The aggregate size of the same four organs is . .	10·002
"Remains for the other 31 organs of the Scotchmen, .	3·692
"The size of the whole head in Mr Stone's Irishmen is .	13·631
"The aggregate size of the same four organs is . .	10·124
"Remains for 31 organs of Irishmen, ...	3·507

Such then is a specimen of "one of the most efficient knock-down blows which Phrenology has yet received." Nothing but the most unreasoning eagerness to disparage Phrenology without one moment's consideration of the grounds for doing so, could have prompted the editor of a respectable literary journal to become the champion of such nonsense as this; and nothing but the knowledge of the existence of a similar feeling in the public mind, could have rendered it safe for him to do so.

mals, under the varying circumstances of Age, Sex, and Country, by Sir William Hamilton, which, as usual, when coming from some notorious anti-phrenologist, goes to invalidate many of the general statements of Gall, and others of the same school. And, truly, the data display a very imposing aspect. "The conclusions," says Sir William, "are founded on an induction drawn from above sixty human brains, from nearly 300 human skulls of determined sex, the capacity of which, by a method I devised, was taken in sand, and the original weight of the brain thus recovered, and from more than 700 brains of different animals."

All this would appear, surely, to afford materials for determinate conclusions; Leuret's forty-three horses sink into absolute insignificance by the side of Sir William Hamilton's pompous parade of facts and figures.

The first conclusion deduced from the premises in question, is, that the adult male encephalon is somewhat heavier than that of the female,—a fact accordant with the statements of Gall, and with the results generally obtained by observers.

The next conclusion is, that, in man, the encephalon reaches its full size about the age of seven. This proposition, if true, would make nothing against phrenology, because, even if the brain should attain its full *size* in early childhood, the organic *quality* is not at that time matured, so that full mental vigour would not be predicated. But Gall thought, and stated his belief, that the full development, in the human species, did not take place till nearly forty years of age; and, by discrediting Gall's accuracy in this particular, Sir William Hamilton, doubtless, expected to discredit him in other respects. Yet, other weighings, conducted on much more extensive materials than Sir William's, lead to a conclusion more nearly approaching to that of Gall; this will be seen upon coming to the figures of Dr John Reid, and some others,—no phrenologists.

The third proposition is opposed to the opinion formed by Gall, and generally adopted by his disciples, that the cranial contents usually diminish in old age. "The vulgar opinion that they do," says Sir W. Hamilton, "rests on no adequate evidence, and my induction would prove the negative." "Adequate evidence," however, shall be adduced in the sequel (so far as figures representing the weights of different brains at different ages can form such evidence), to show at least the high probability of this "vulgar opinion."

Another induction obtained by Sir W. Hamilton is expressed as follows:—"The opinion that the African brain, and, in particular, that of the Negro, is greatly smaller than that of the European, is false. By a comparison of the capacity of two Caffre skulls, male and female, and of thirteen Negro crania (six male, five female, and two of doubtful sex), the encephalos of the African was found not inferior to the average size of the European." How far this series of weights and measurements corresponds with others relating to the same subject, and on a larger scale, will be seen shortly.

"In Man," we are further told, "the cerebellum, in relation to the brain proper, comes to its full proportion about three years. This anti-phrenological fact is proved by a great induction." The sources of fallacy incident to the employment of a small number of instances, with respect to any such proposition, are obvious enough.

Again: "The cerebellum, in the female, is in general considerably larger, in proportion to the brain proper, than in the male. In the human subject, the former is nearly as 1 to 7·6; the latter nearly as 1 to 8·4; and this sexual difference appears to be more determinate in man than in most other animals. Almost the whole difference of weight between the male and female encephali lies in the brain proper; the cerebella of the two sexes absolutely are nearly equal,—the preponderance rather in favour of the women. This observation is new, and the truth of the phrenological

doctrine implies the reverse." This " new observation" has, at any rate, received no corroboration. Dr John Reid's Tables, and those also of Professor Tiedemann, exhibit results affecting this matter, in perfect harmony with " the truth of the phrenological doctrine," and entirely opposed to the facts of Sir William Hamilton.

Such are the leading deductions gained by this observer from weights and measurements ; and what is extraordinary is, that whenever any particular result would affect very sensibly the accuracy of the phrenologists, this distinguished opponent never fails to come at it; he never exhibits a lack of figures and facts that favour, decisively, his own anti-phrenological notions. The present writer has no means of forming a judgment respecting the cause of this circumstance; whether it may have been owing to accident incidental to insufficient data, or to some *prejudiced carelessness* in the mode of his proceeding, he offers no opinion. The fact itself, however, is curious.

A still more remarkable circumstance is to be discovered in regard to certain statements of Professor Tiedemann of Heidelberg, founded on certain researches of this kind, and contained in a communication published in the *Philosophical Transactions* for 1836, " On the brain of the negro compared with that of the European and the Ouran Outang." Tiedemann has long been known, and committed, as an opponent of the cerebral physiology of his fellow-countryman, Gall, who, in illustration and confirmation of some of his positions, had appealed, in a general way, to the small size of the African head, as a fact corresponding with the slender endowment of psychical energy, when contrasted with that which obtains in the European. It did not appear to be a prominent object with the Professor to interfere with Gall's physiology: it cannot even be said with certainty that he intended to scathe the same by a dart obliquely cast. It is certain, however, that he advanced, as results of weighing and measuring, inductions having such a ten-

dency. His particular purpose was to prove that the Negro race was behind the European nations, neither in general capacity, nor in educability; he having adopted the opinion, that human perfectibility is in relation to the capacity of the cranial cavity. The most singular business of all about this business is, that the Professor actually published a conclusion at variance with his own figures! And yet, strange to say, the Royal Society, and half of our scientific men and journals, not only failed to discover this error, but received and propagated his facts and inferences as literally correct, and as of vast importance. The men of science of this generation, in their eagerness to hail any evidence or argument apparently calculated to refute phrenology, have, on more than one occasion, actually stultified themselves by receiving and commending self-evident errors and inconsistencies when adduced against it. Well might Broussais observe, in reference to assumed anti-phrenological facts: "I do not know to what extent such alleged facts merit our confidence. For my own part, I declare that they will inspire me with none, until they shall have been verified by phrenologists."

The merit of detecting, and of exposing, the fallacies occurring in Tiedemann's communication to the Royal Society, belongs to Dr A. Combe, who, in a paper, published in the eleventh volume of the Phrenological Journal, well and clearly showed the inconsistency of the figures with the inductions; and, as this matter possesses considerable interest in exhibiting the little reliance to be placed on anti-phrenological statements, even when assumed to be supported by weights and measurements, abstracts of Dr Combe's valuable communication shall here be cited at some length.

"Tiedemann's grand objects are, to prove, 1st, that the opinion of Negro inferiority expressed by Camper, Sœmmering, Cuvier, and almost all naturalists of any eminence, is incorrect; 2dly, that the Negro brain is equal in size

and similar in structure to that of the European; and, 3dly, that, consequently, the former is equally capable of civilization as the latter, and owes his present inferiority entirely to bad treatment and unfavourable circumstances, and will lose it when placed in the position in society which has been recently assigned him by the 'noble British Government.' . . . . . . It is obvious, that of two brains, both precisely equal in absolute weight, one may be very deficient in intellectual endowment, compared with the other, and this deficiency be perfectly apparent, on inspection, when we attend to the regions of the brain in which the preponderance lies. But as Tiedemann, throughout the whole of his experiments, utterly disregards this distinction, confounds intellectual power, moral feeling, and brute propensity, under one head, and treats of the brain as if it consisted only of one lobe, with only one function, namely, the manifestation of intellect, his inference, that because the Negro brain is equal in weight to the European, therefore the Negro is also his equal in intellectual power, falls to the ground, as unwarranted by the evidence. To render his conclusions worth any thing, he must show, not only that the two brains are equal in absolute size, but that the *anterior lobe*, or *seat of intellect*, is equally developed in both;—a position which he never attempts to substantiate, and which is at variance with some parts even of his own facts."

"Having obtained the weight of a sufficient number of European brains, Tiedemann next endeavours to ascertain the weight of the Negro brain; but, from the very small number of Negroes to be found in Europe, he has great difficulty in obtaining any thing like a fair average: in fact, he gives the weight of *only four* Negro brains,—one of a boy of fourteen years of age, stated, on the authority of Sœmmering, to have weighed 3 lb. 6 oz. 6 dr.; a second, of a tall and handsome Negro of twenty years of age, which weighed 3 lb. 9 oz. 4 dr.; a third, of a large Negro, men-

tioned by Sir Astley Cooper, of 49 oz.; and a fourth, examined by himself, of a man twenty-five years of age, which weighed 2 lb. 3 oz. 2 dr."

"In comparing these results with the average weight of the European brain, as stated by Tiedemann himself, it is singular to observe the extent to which they are at variance with his inferences. The European average runs, he says, from 3 lb. 2 oz. to 4 lb. 6 oz., while the average of the four Negroes rises to only 3 lb. 5 oz. 1 dr., or 3 oz. above the *lowest* European averages; and the *highest* Negro falls 5 oz. short of the highest *average* European, and no less than 10 oz. short of Cuvier's brain. And, as if these facts were not inconsistent enough with his conclusions, Tiedemann first affirms that, in the Negro, "the length and height of the cerebral hemispheres do not visibly differ from that of the European; their breadth being only somewhat less," (p. 515); and, immediately after subjoins three tables of the "Dimensions of the Cerebrum of Negroes," "Dimensions of the Cerebrum of European Males," and "Dimensions of the Cerebrum of European Females;" the figures of which directly contradict his assertions! This seems almost incredible, but on summing up the averages we find the following results, namely:—

			Inches.	Lines.
"Average length of brain in		4 Negroes, . .	5	11
Do.	do. .	7 European males, .	6	$2\frac{1}{7}$
Do.	do. .	6 European females .	5	$10\frac{1}{2}$
"Average greatest breadth in		4 Negroes, . .	4	$8^{\bar{6}}$
Do.	do .	7 European males, .	5	$1\frac{1}{7}$
Do.	do. .	5 European females,	5 [1]	$4\frac{1}{3}$
"Average height of brain in .		3 Negroes, . .	2	$11\frac{1}{3}$
Do.	do. .	7 European males, .	3	4
Do.	do. .	4 European females,	2	$9\frac{1}{2}$

[1] "This result is an example of the unsatisfactory mode of proceeding pursued by Tiedemann. It makes the breadth of the female *greater* than that of the *male* brain, in opposition to all his other evidence, and must have arisen from the unequal spreading out of the soft brain; if, indeed, it is not a mere error of the press."—Dr Combe.

"From these tables, it is evident that the dimensions of the brain are smaller in the negroes measured by Tiedemann, than in the European; but, for our own parts, we are not disposed to lay much stress upon results drawn from such a limited number of facts; and we notice them merely to show that, such as they are, they directly contradict the arithmetical proportions or conclusions drawn from them by Tiedemann." . . . .

"Not having access to a sufficient number of the actual brains of negroes, Tiedemann has endeavoured to supply the want of direct evidence, by comparing the capacity of the negro skull with that of the European, and thus obtaining an index to the relative size of their contained organs. For this purpose he filled the skulls with millet seed, and carefully noted the quantity which each contained. . . . . Among other collections which he visited, was the phrenological museum in Edinburgh, to which we twice accompanied him, and from which twenty-three of his observations were taken; and we can bear witness to the pains which he took to ensure accuracy in the individual details; and yet, strange to say, on summing up his table of results, and striking an average (a proceeding which he seems not to have thought of), HIS FACTS AND INFERENCES ARE ONCE MORE AT UTTER VARIANCE. . . . . After giving several pages of tables comprehending the weight of the quantity of millet seed required to fill Ethiopian, Caucasian, Mongolian, American, and Malayan skulls, Tiedemann says, 'It is evident from the comparison of the *cavum cranii* of the negro with that of the European, Mongolian, American, and Malayan, that the cavity of the skull of the negro, in general, is *not smaller* than that of the European and other human races. The result of Hamilton's researches is the same. I hope this will convince others, that the opinion of many naturalists, such as Camper, Sœmmering, Cuvier, Lawrence, and Virey, that the negro has a smaller skull and brain

than the European, is *ill-founded, and entirely refuted by my researches*' (p. 511). Now we have already seen that the real question of interest, as regards negro improvement, is not so much the general size of the brain, as the relative size of its anterior lobe and coronal surface compared to the basilar and posterior portions. But even as concerns the absolute size of the whole brain, it is an extraordinary fact, that Tiedemann's own tables give a decided superiority to the European over the negro brain, to the average extent of nearly four ounces! The average capacity of forty-one negro skulls in his own tables amounts only to 37 oz. 1 dr. 10 gr., while the average of seventy-seven Europeans of every nation, also in his own tables, amount to 41 oz. 2 dr. 30 grains. Of the negroes, indeed, three are females, but even subtracting these, the negro average amounts only to 37 oz. 6 dr. 18 grains.[1] Here, then, on Tiedemann's own showing, we have, first, an inferiority in the dimensions of the negro brain, and a greater narrowness of its anterior lobe; and, secondly, a marked inferiority in the capacity of the negro skull, to the extent of about one-tenth, and yet he very strangely infers that *both are equal* to those of the European. . . . . If the phrenologists had perpetrated such a series of blunders, Sir William Hamilton and his allies would have shouted in triumph over their stupidity."

After all this, is it very unnatural that phrenologists should receive with distrust any statement of facts which comes from opponents, though backed, professedly, by weighing and measuring? When it is seen that some, in their zeal to destroy phrenology, will not scruple to make statements which are positively untrue, and based upon a method of investigation absurd in itself and preposterously

[1] Thus it appears that Tiedemann's *facts* do *not* corroborate Sir William Hamilton's inductions relating to the African brain, as he himself thought; but that, on the contrary, they are most conclusively opposed to them.

vicious, as in the case of Mr Stone; when others, like Sir William Hamilton, advance results of weight and measurement opposed to every body's experience but their own, and thereby force the conclusion that fallacy must lurk somewhere; and when it is found that one so eminent as Tiedemann can offer as valid inference that which his own premises confute,—is it unnatural, again let it be asked, that the correctness of Leuret regarding the horse's brain should at least be doubted? Moreover, the remarkable circumstance in Leuret's statements should not be overlooked, that, on all points, the result is most happily made to come out in extreme opposition to what Gall had set forth. Thus, according to the observations of phrenologists, the females of species generally, being less ardent in sexual feeling than the male, possess, in the majority of instances, a smaller cerebellum correspondently, yet Leuret makes the cerebellum of the mare to exceed that of the unmutilated horse. Again, Gall, Vimont, and others, have recorded cases in which removal of the testes led to atrophy of the encephalic organ, and the circumstance has been considered by phrenologists to be probably not an unfrequent fact; yet Leuret makes the cerebellum in the gelding, to *surpass* in weight that either of the mare or of the stallion. The fact, that averages of so small a number of examples should, in each little particular, run just counter to all phrenological statements, is in itself a somewhat suspicious affair. But M. Leuret has an anti-phrenological reputation to maintain!

Let us now proceed to examine the result of certain other weighings and measurings on the part of Tiedemann, which, so far as can be judged, were conducted without bias, and without any seeming view either to controvert or to corroborate the observations of Gall and his followers. Under such circumstances, it may be noticed, almost universally, that the results constitute a confirmation of the phrenological observations, and, certainly, that

they nowhere exhibit any very material discordance with them.

However opposed to truth the *inferences* of Tiedemann may have been, no suspicion can attach to his good faith, or to the accuracy of his *facts*. Dr A. Combe, it will have been seen from the foregoing extracts, personally testifies to the scrupulous fidelity with which, in his own observation, he prosecuted his inquiries in the procuring of data. It is no wonder, then, to the phrenologist, that, in point of fact, all his figures do, *pro tanto*, support the statements of Gall, notwithstanding that his wish and expectation were, in all probability, quite the other way. This, to some extent, will have been apparent from what has already been advanced; and it may still further be shown from examination of some others of his results.

Tiedemann says,—"The brain of men who have distinguished themselves by their great talents is often very large. The brain of the celebrated Cuvier weighed 4 lb. 11 oz. 4 dr. 30 gr. troy, and that of the celebrated surgeon, Dupuytren, weighed 4 lb. 10 oz. troy. The brain of men endowed with but feeble intellectual powers is, on the contrary, often very small, particularly in congenital idiotism." In another place, speaking of certain tribes who inhabit the interior of Africa, as being far more intellectually constituted than those on the coast, he says, "Most of them have well *formed* skulls;" and again, "the Caffres and Bachapins, or Betchuanas, have the *same form of skull, and the same high forehead* and prominent nose, as the European." Sir William Hamilton, in his onslaught on phrenology by the agency of tables, expected to have struck a fatal blow, when he made it out that the development of the cerebellum is much smaller in males than in females, and that the brain sustains no diminution in old age, as Gall had asserted. Tiedemann, however, shows, by *his* tables, that not only the female brain, but the female cerebellum also, is decidedly smaller.

In his list of "conclusions," he expressly affirms that "the female brain is lighter than that of the male, and weighs, on an average, from four to eight ounces less." In another of his conclusions, after remarking that Sir William Hamilton denies the decrease of the brain in old age, he observes, "It is very remarkable that the brain of a man eighty-two years old was very small, and weighed but 3 lb. 2 oz. 3 dr., and the brain of a woman about eighty years old weighed but 2 lb. 9 oz. 1 dr. I have generally found the cavity of the skull smaller in old men than in middle-aged persons. It appears to me, therefore, probable, that the brain really decreases in old age, only *more remarkably in some persons than in others;*" just as may happen under circumstances of emasculation in horses;—decrease of the cerebellum will *probably* happen; but may not this occur *more remarkably in some geldings than in others?* This should be kept in mind, in every estimate of Leuret's facts.

"Again, in regard to Sir William Hamilton's assertion of the equal or superior size of the female cerebellum," says Dr A. Combe, "we find, at page 514, a table entitled, 'Dimensions of the Cerebellum and Nodus Encephali.' In it, the greatest breadth of the cerebellum in six male Europeans, varies from 4 inches 3 lines, to 3 inches 6 lines, being the highest and lowest measurements. But in three female Europeans, the *highest* is only equal to the *lowest* male, namely, 3 inches 6 lines; the other being 3 inches, 5 lines, and 3 inches 3 lines respectively. Here, again, the superior accuracy of the phrenologists is proved even by hostile testimony."

M. Parchappe, physician to the lunatic asylum of Rouen, proposed in the work whose title has been previously mentioned, to estimate, by weights and measurements, the relations subsisting between the intellectual faculties and the size of the brain. His proceeding being, first, to measure the head in the living subject, and then to measure the head, and to weigh the brain of the same individual; noticing, at

the same time, every circumstance of sex, age, stature, health, intellect, and so on, which was calculated to throw any light upon the subject. The facts observed by the author amount to 344; or 169 heads measured, 58 skulls measured, 22 skulls weighed, and 95 brains weighed. M. Parchappe is no phrenologist; and he has consequently overlooked, in his investigations, several conditions that might have illustrated the phrenological question to a much greater extent than his actual results appear to do; nevertheless, on reviewing the conclusions, it will be seen that, in the main, they substantiate the accuracy of Gall's observations, and that they, in no instance, manifest opposition to the principles of his physiology. The leading inferences, drawn from the facts regarding the relations of size of brain, and development of the intelligence, are as stated below:—

Size of Head and Brain. This is much smaller in the female than in the male, not only *en masse*, but in all the separate diameters. The weight of the cranium also is less in the female. This greater development of brain in the male than in the female has long been maintained by several writers, as a fact coincident with the less energetic constitution of mind in the latter.

Development of Intelligenge. The intelligence bears no proportion to the size of the head in fools and idiots. But on comparing the average of size of ten heads of men of superior intellect with that of ten heads of persons whose faculties were below par, the advantages were clearly on the side of the former. In men, a certain size of head is necessary for a proper development of the intellect; but beyond this we find no necessary connexion between the volume of the head and the development of the intellect.

The statements in the latter paragraph harmonise entirely with the recorded experience of every phrenologist. The intelligence does not bear a relation to the size of the whole head (as some physiologists, placing this property in the entire cerebrum, would seem to hold), but to the size of its

anterior division. As a very frequent fact, however, distinguished intellect is found to be associated with a large aggregate head, mainly on account of the greater height and breadth of the forehead, so that the results of striking an average of the absolute magnitude of ten intellectual heads, and comparing it with a corresponding average of ten weak-minded persons, the phrenologist would always expect to constitute an advantage "clearly on the side of the former." "In men, a certain size of the head is necessary for a proper development of the intellect," because a head below a certain size is always found to possess, necessarily, a feebly developed forehead.

It will be recollected that Sir William Hamilton asserts that, "In man, the encephalos reaches its full size about seven years;" advancing this proposition upon the strength of his weights and measurements, he very complacently goes on to say, "This was never before proved. The Wenzels rashly generalised from *two* cases the conclusion, that the brain reaches its full size about seven years; as Sœmmering had, in like manner, on a *single* case, assumed that it attains its last growth by three. Gall and Spurzheim, on the other hand, assert that the increase of the encephalos is only terminated about forty. The result of my induction is deduced from an average of thirty-six brains and skulls of children, compared with an average of several hundred brains and skulls of adults." Let us now see to what results the balance and the rule led M. Parchappe; these correspond much more nearly with the opinions of Gall, than with the positive statements of Sir William Hamilton. According to the French observer, the volume of the head does not appear to be limited by the period at which the general growth of the body ceases; the head seems to enlarge gradually up to the age of sixty years. The increase of size shows itself almost exclusively in the horizontal, circular development of the head, and, in the later period, depends chiefly on enlargement of the frontal sinuses.

After sixty years of age, the size of the head diminishes; the weight of the skull also diminishes in old age. M. Parchappe's observations on the brain, considered apart from its cranial investments, lead him to conclude that the brain continues to increase up to the age of forty, that it remains stationary to seventy, and then begins to decline. And these results too, it must be kept in mind, come from weighing and measuring.

Dr Morton, professor of anatomy in Pennsylvania college, Philadelphia, has published, in a work entitled *Crania Americana*, the results of numerous weighings and measurements, which, undertaken without any express regard to phrenology, strikingly confirm some of its leading truths, in so far as these latter can be affected by such methods of investigation. Certain phrenological measurements, however, of a large number of crania, taken by Mr Phillips, are introduced into the work, though Dr Morton himself scarcely alludes to phrenology in his own descriptions of the crania, and gives only a qualified assent to "the details of cranioscopy as taught by Dr Gall, and supported and extended by subsequent observers." The general conclusions, however, to be drawn from the facts recorded in the work, are certainly corroborative of Gall's physiology, so far as they go. The native American head is shewn to be decidedly inferior to the average Europeau head, a fact which indisputably coincides with their psychical differences. The internal capacity of cranium, in the five principal races of mankind, is stated in cubic inches, in the subjoined table.

	No. of Skulls.	Mean Internal Capacity.	Largest in the series.	Smallest in the series.
Caucasian,	52	87	109	75
Mongolian,	10	83	93	69
Malay,	18	81	89	64
American,	147	80	100	60
Ethiopian,	29	78	94	65

The Caucasians were, with one exception, derived from

the lowest and least educated class of society. Distinguished into national races, they consisted of the following:—

Anglo-American,	6
Germans, Swiss, Dutch,	7
Celtic, Irish, Scots,	7
English,	4
Guanche (Libyan),	1
Spanish,	1
Hindoo,	3
Europeans (unascertained nations),	23
	52

The Mongolians were seven Chinese and three Esquimaux. The Malays were thirteen Malays proper, and five Polynesians. The Ethiopians were all unmixed negroes, nine of them native Africans. The Americans were of both the two leading races, as recognised by Dr Morton, namely, the Toltecan, and the barbarous nations.

In the course of the work, a series of measurements is given of the several regions, rudely fitted for exhibiting the relative capacity of the several divisions of the cranial cavity, corresponding with the development of different portions of the brain, the general result of which is such as the phrenologist would anticipate; but so much possible fallacy is involved in all yet ascertained methods of proceeding in this respect, that the present writer is not disposed to place very great confidence in any conclusions thus obtained; moreover, as the measurements were taken by Mr Phillips, a professed phrenologist, an introduction of them here would not strengthen the argument, which is to show, that all weighings and measurings done by individuals previously uncommitted upon either side of the controversy, have always resulted, mainly, in corroboration of Gall. Dr Morton himself is, or at least was, neuter; hence value may attach to a statement which he makes, in a prefatory letter to Mr Phillips, as follows,—" I am free to acknowledge that there is a singular harmony between the mental

character of the Indian, and his cranial developments, as explained by phrenology."

Dr John Reid, after weighing so many as 253 brains under all sorts of circumstances, supplies conclusions which, although they do not in all respects coincide with the statements of Gall, no where contradict the principles of his physiology; whilst, in several particulars, they are directly opposed to the inductions of Sir William Hamilton, the only authority (omitting Leuret) whose *figures* appear to weaken the substantial accuracy of the phrenologists; for, as regards Tiedemann, it will have been seen that his figures are with Gall, though the inferences deduced from them by himself, are the other way; and, as to Mr Stone, *his* doings have long ago sunk beneath contempt. Now then, let us see the results obtained by Dr Reid; let us contrast them with those of Sir William Hamilton on the one hand, and with the statements of Dr Gall on the other.

Sir William stated, that " the encephalos reaches its full size about seven years of age, Gall had said, between the ages of twenty and thirty, and in some individuals not till forty; Dr Reid states the average weight of the encephalon, at the age from five to seven, to be 43 oz. 10 dr.; from twenty to thirty, 50 oz. $9\frac{1}{2}$ dr.; Sir William Hamilton said, that his inductions prove that the cranial contents do not decrease in old age, as Gall had asserted; Dr Reid states the average weight of the encephalon, between sixteen and twenty years of age. to be 52 oz. $10\frac{2}{3}$ dr., and at seventy and upwards, 48 oz. $4\frac{2}{7}$ dr.

Sir William Hamilton propounded, not only that the cerebellum is much larger in relation to the cerebrum in the female, than it is in the male, but even that it is *absolutely* larger. Dr Reid reports the female cerebellum to be decidedly lighter at every age; thus, at the ages from five to seven, this organ in the male weighs 4 oz. 7 dr.. and in the female 3 oz. 11 dr.; from sixteen to twenty, in

the one case, its weight is 5 oz. 4½ dr., and in the latter 4 oz.

Sir William Hamilton affirmed, that the cerebellum, in man, attains its full development in relation to the brain, at about three years of age. "This anti-phrenological fact," says he, "is proved by a great induction." Dr Reid, however, in a table representing the "Relative Weights of Encephalon to Cerebellum," in 172 cases, gives the average proportion in the male, at the ages between one and five, as 1 to 10 2-5ths, and, between thirteen and fifteen, as 1 to 9 1-11th; and a similar proportion is given for the female. The reader will now judge upon which side of the phrenological controversy the weights of Dr Reid preponderate,—whether they favour more the inductions of Hamilton, or the statements of Gall.

A great deal of the preceding may seem to the reader to be, what in point of fact it is, very much of a digression from the professed object of the present chapter. It may appear to some, that all these discussions regarding the physical characteristics of the brain and the skull would have occurred more appropriately in the previous chapter, the professed purpose of which was, to show the harmony subsisting between the anatomy and the physiology of Gall. Yet, the necessity that existed for a different course may at once strike the reader, who will readily appreciate the reasons for this seeming digression. It has been the author's intention to exhibit, in the first place, the absence of all discordance between phrenology and the effects of mutilations of the encephalon, and then, the actual corroborations of it which many of this class of facts afford. In the prosecution of this design, it became necessary to adduce the recorded instances attesting the reciprocal influence of lesions of the cerebellum, and of the external organs of generation; and especially, to direct attention to the cases evincing marked effects upon the cerebellum from removal of the testes. In relation to these latter facts, certain weighings of the brain

and cerebellum in the horse, under the different circumstances of sex and emasculation, were considered by some persons to have antagonised any value which might otherwise have belonged to them; and not only M. Leuret, but several influential physiologists also in this country, have of late cited the weights in question as being all but destructive of Gall's physiology of the *cerebellum*. How were the objections based upon M. Leuret's facts to be met? First of all, by showing that they really furnish no decisive objection at all, even if no inaccuracy have found its way into the proceeding adopted in collecting them. But still their tendency, if fairly representing the average state of things, must undoubtedly be allowed to be towards neutralising much of the evidence gathered together in favour of the sexual functions of the cerebellum; and, how was it to be shown that, as demonstrating a general fact, no reliance whatever is to be placed upon them? Doubtless, it will be said, by accumulating corresponding instances, in equal, or in greater number, that may fairly antagonise those of M. Leuret. All this is very easily said; but, to render any aggregate of such facts of the slightest worth,—available for any conclusion,—some hundreds of instances, at least, would have to be collected. And what possible means exist for any ordinary individual to accomplish such a purpose? To obtain, indeed, a very limited number of examples would be a work of extreme difficulty, and the result would be but little likely to compensate any inquirer for the pains he might take, whatever condition of things should be revealed. In this view of the question, the author deemed it to be the best proceeding to detail, at some length, the circumstances which deprive all such figures as those of M. Leuret of any value in his own estimation, and to leave such circumstances to produce their natural effects upon the mind of the reader. From the facts which have been set forth, the fallacious and contradictory character of any conclusions deduced from a limited number of cases, must

have been rendered obvious. The necessarily inconclusive results taken from only forty-three observations,—the number of horses' brains referred to by Leuret,—ought, surely, to carry with them no weight. Of the utter insufficiency of such data, no one seems to be better aware than Dr John Reid, who, in his communication to the London and Edinburgh Monthly Journal of Medical Science, for April 1843, wherein his communication upon this matter first appeared, says:—" No one can be more perfectly satisfied than myself, that though, at first sight, the data I have amassed appear sufficiently ample to enable us to draw satisfactory conclusions regarding the average weight of some of the most important organs of the body at different periods of life; yet that they are, when more narrowly examined, much too scanty for the purposes intended." And this is said in regard to 253 examples, nearly six times the number of Leuret's horses. Respecting the disturbing influences of abnormal conditions on the weight of structure, Dr Reid notices the impossibility of arriving at sure conclusions, in the absence of far more extensive materials than had been at his command. " The accumulation," says he, " of individual facts on the subject with which we have at present to do, is chiefly valuable in pointing out the great variety in the weight of the same organs in a state of health in different individuals of the same age,—thus enforcing upon us the sources of fallacy to which we are liable in drawing our averages from a small number of cases, and impressing upon us the insufficiency of any comparison between the weight of a diseased organ in any individual case, and the *average weight* of a healthy organ at the same period of life, in enabling us to form any correct estimate of the change in weight which it has undergone in consequence of morbid action, or other causes."

It has been already advanced that Gall's experience, that of Vimont, and of some others, in reference to degeneracy of the cerebellum after castration, regarded *volume*,

not *weight;* as before observed, we have no certainty that, even in Leuret's instances, the higher average of weight in the cerebellum of the twenty-one geldings was correspondent with greater volume; certainly, this is not affirmed. Yet, whatever were the actual facts of the case, the phrenological question is entirely unaffected by them, on account of the many sources of fallacy which they include, the chief of which, undoubtedly, is their limited number.

The author would just venture to observe, before taking leave of this subject, that from past experience the advocates of phrenology are justified in withholding assent altogether from such facts as those of M. Leuret, so long as they are unconfirmed by less partial individuals. Misstatements, on the part of opponents, have more than once been detected; and, when we consider the difficulty of repeating such experiments as those in question, it is seen at once how they must have furnished unusual *temptation* to strain a point. This *may* have been done, in the process of separating the actual cerebellum from its attachments, isolating it very completely when a light weight was hoped for, and doing this but imperfectly when the contrary state of things was expected and wished. This, however, is but thrown out as a hint for caution in the estimate of such experiments. To some persons, the merest suggestion of possible inaccuracy through bad faith, may appear to be in any thing but good taste; and it may, in point of fact, be in very bad taste; yet, in the case of phrenology, it is justified by past experience.[1]

[1] It may here be stated, that M. Leuret himself does not furnish the guarantee with regard to the correctness of the observations upon which the author has commented so much in the text; the weighing of the brains of the forty-three horses took place at his suggestion, but was executed by M. Lassaigne. The writer is not sufficiently acquainted with the reputation of this latter gentleman, to form any opinion as to how far the character for accuracy, or for fidelity, in the proceedings in question, may be influenced by this circumstance.

In bringing the present chapter to a close, the author may observe, in recapitulation, that it has been his principal object to prove two things; *first*, That no recorded facts concerning mutilations of the encephalon are at variance either with the principles or with the leading details of the cerebral physiology taught by Gall—but that, on the contrary, they harmonise most completely with this physiology, when rightly interpreted; and, *secondly*, That, whatever be the true state of matters as to any possible function in the cerebellum affecting locomotion, vivisections have established nothing of the kind. The discussion has been extended in the case of the cerebellum, because opponents, of late years, have levelled their whole force at this particular point of Gall's doctrine; and because, in opposition to it, much plausible evidence has been accumulated, which, however, on careful examination, shows that *plausibility* is the highest commendation it ought to receive.

It was stated, at the commencement of this chapter, that mutilations of the cerebral hemispheres had, apparently, been given up, as making either for or against phrenology; nevertheless, at times, it will be feebly reiterated that large portions of the cerebrum have been removed by accident, and yet that no corresponding disturbance, or diminution, of the mental manifestations, has been witnessed as a permanent consequence. To this it may be said, that, if such facts have any discordance with phrenology, they exist in exactly the same relation to every physiology which recognises the brain to be the organ of the mind. Whenever it shall be shown that the whole cerebral structure, in each hemisphere, associated in the phrenological doctrine with some particular faculty or quality, has been removed, and that the said faculty or quality of mind has yet continued to be displayed, the functions of *one* phrenological organ will be disproved; yet, even in such a case (which has never yet occurred), phrenology, as a true physiology of the

brain, would yet remain. With respect, however, to all such circumstances of cerebral mutilation, doubt must nearly always prevail as to their true bearings, for reasons already adduced in an earlier chapter upon the same subject. The detailed argument need not be repeated.

CHAPTER IX.

HARMONY OF GALL'S PHYSIOLOGY WITH COMPARATIVE ANATOMY.

The remarkable and striking analogies which are observable amongst various tribes in the animal kingdom, and particularly in regard to the constitution of the nervous system, have long been subjects upon which philosophers have delighted to expatiate. In the several circumstances under which these analogies develope themselves, it would be difficult to discover more complete and decided correspondences than those which constitute the harmony subsisting between the physiology of Gall and the leading facts of comparative anatomy. A few words will render the general truth apparent.

According to the physiology in question, it has been shown in the foregoing pages, not only that the brain is the organ of the mind, but that it is a congeries of organs, the function of each being to display some special faculty of the mind; the cerebral convolutions maintaining a characteristic relation to the power and multiplicity of the psychical attributes. It has further been shown, that those convolutions which, in man, are placed in the posterior region of the cranial cavity, are proved by numberless observations to form the organic instruments of the more animal dispositions of the species; that those which are situated anteriorly, constitute the intellectual organs; and that the structures located between the posterior and anterior lobes of brain, are for the manifestation, in this life,

of the moral and religious tendencies of human nature, in so far as these depend upon certain inward feelings or sentiments. Now, comparative anatomy demonstrates the existence, in the entire animal kingdom, of a constitution of brain exactly correspondent with such a state of things. The harmony, indeed, of Gall's physiology with every thing that is known of cerebral anatomy, is so striking, that no one who examines this subject, free from bias, can fail to recognise it at once. The circumstance is obvious to every observer. "A remarkable gradation," say Todd and Bowman in their recent work,[1] "is observable as regards the number of the cerebral convolutions from the lowest mammalia up to man. Some of the Rodentia, Cheiroptera, and Insectivora, occupy the lowest place; and monkeys, the elephant, and the whale, rank next to man, in whom the convolutions reach their highest point of development."

It is certain that, in those species where the presence of a structure analogous to the cerebral hemispheres is really wanting, there is no clear or certain manifestation of the principle of consciousness; in fishes, where undoubted evidence of this to a slight extent is noticed, an extremely simple development of brain is discovered; in birds, where an obvious increase of psychical energy takes place, a correspondingly higher type obtains in the encephalon, rudiments of the convoluted character in some instances exhibiting themselves; and, lastly, in pursuing the anatomy of the brain through whole classes or mammalia up to man himself, a continuous illustration of the same fact is to be found.

Gall's physiology shows that the anterior and superior portions of the human brain are associated with the more exclusively human attributes; and it is notorious that, in these regions, convolutions develope themselves more and more as we ascend in the scale, and attain their highest per-

[1] P. 280.

fection and greatest complexity in man himself. "When the brain," say Todd and Bowman, "has acquired an enormous increase of size, as in the elephant and in man, new convolutions seem to be added to the primary ones met with in inferior groups, and the secondary folds are greatly increased in number. *The additional folds are found chiefly at the superior and anterior part of the hemispheres.*" Thus distinctly, in all its material details, is the cerebral anatomy of Gall admitted, even by those who reject his *physiology*. Yet the present writer would suggest, that the opponents of this latter should of all others be the most scrupulously honourable in doing justice to Gall,—in attributing to its original author that which they do *not* reject. It is really most unfair that Gall and Spurzheim should be so slightly passed over as they are in many modern anatomical works. This proceeding, indeed, has been the besetting sin of anti-phrenological anatomists from Reil downwards; there have been a few honourable exceptions; but, more generally, the anatomy has been appropriated without any open and distinct acknowledgment.

So very complete is the harmony of comparative anatomy with the doctrine of phrenology, that it is no uncommon circumstance to find, that a conviction of the truth of this latter has resulted mainly from the evidence which the former affords. When this is the case, however, the philosophy of the question chiefly discussed in this work—the method of investigation, and the value of evidence—is entirely unaffected. Because some phrenologists largely appeal to the facts of comparative anatomy for *proofs* of the doctrine advocated, they are not the less inadequate as primary data; and, in like manner, when the opponents cite corresponding phenomena in its *disproof*, they virtually waste their strength; because, in the very nature of things, *analogies*, however forcible they may seem, can never antagonise *inductions* legitimately procured by direct observation. Yet, undoubtedly, from the very constitution of

certain minds, analogies, and secondary proofs generally, will seize upon the understanding more readily and firmly than rigorous and more direct demonstrations.

And yet the attempt to confute phrenology by weakening its secondary evidences is a proceeding constantly adopted. Under such circumstances, there is rarely any disposition to deal with the direct fact, or even to dispute the conclusions in the event of accuracy in the premises; but individuals of all degrees of standing will advance unceasingly, that particular phenomena, recognised in the animal kingdom, prove phrenology to be untrue, because, *in their estimation*, they do not correspond with it. A thing is to be rejected, because certain other things, respecting whose true import knowledge is at the best uncertain, do not *appear* to be in harmony.

A happy illustration of this procedure presents itself to the author, even while he is engaged upon the present pages. At the Medical Society of the London University College, a paper relating to the present subject, by Mr John Marshall, was read on the 14th November 1845, and an abstract was published in the Lancet for the 29th of the same month. As the paper in question *hashes up* sundry old objections to Gall's physiology *originating* with really able men, but at the present day not very often *repeated*, excepting by inferior persons who have occasion to *get up* an attack upon phrenology, some detailed examination of Mr Marshall's statements shall here be made; more especially since they embody, in some respects (as the present writer has been informed) the objections which Professor Sharpey is in the habit of adducing in his lectures. There is little doubt, however, that the objections, to whomsoever Mr Marshall is immediately indebted for them, are most imperfectly retailed, as the paper itself yields ample evidence that its writer is not at all acquainted with his subject,—no uncommon thing with the out-and-out opponents of phrenology.

Mr Marshall sufficiently justifies this very severe criticism in several places; and nowhere, more than in the following passages, taken from the abstract inserted in the before mentioned number of the Lancet. Introducing the subject of Gall's discoveries, he says: "In pursuing this inquiry, it is important to remember, that the cranioscopical method of determining the existence of a separate organ in the brain, was conceived by Gall in his youth, and adopted when he was yet avowedly unacquainted with its anatomy in man, its mode of development in the fœtus, and its form in the lower animals." And very properly so too, it may be contended, for reasons already stated in other parts of this work. Have not *vital* functions—those of the nerves, for example—been *always* recognised, anteriorly to any precise acquaintance with the anatomy of organs? He then goes on, "Of the two latter subjects (the brain of the fœtus and that of the lower animals), he continued to know comparatively little." This is an assertion made by the author of the paper in ignorance. It is both presumptuous and absurd for persons to speak of what writers have said, and what they knew, without any actual knowledge of their works, satisfied with repeating what they have somewhere read, or heard; for, in justification of these remarks as applicable to Mr Marshall, it is quite certain that he has never studied the writings of Gall, who, as it happens, *did* know a great deal about these "two latter subjects." Next,—"The first, with the aid of Spurzheim, he pursued with assiduity and success; but it is obvious that here anatomy *followed* physiology, instead of having *preceded* it." If this passage have any available meaning, it signifies that dissection conduces primarily to the knowledge of function; now, let Mr Marshall adduce some illustration, by example, of this doctrine; let him take the instance from the spinal cord or the nerves, in order to make the parallel complete. It is folly to talk about determining the offices of the

brain by study of its anatomy, antecedently to recognition of functional manifestation. Again,—" He made no successful application of his anatomical researches to the main idea of his system." This is simply untrue.

Our essayist then comes to the point wherein the particular argument of the present chapter is involved, and goes on to say :—" The cranioscopical observations of Gall, Vimont, and other phrenologists, in the lower animals, based essentially on the form of the skull and its cavity, are inconclusive, inasmuch as their knowledge of the convolutions was too incomplete to enable them to assign precise limits to their assumed cerebral organs, or to identfy equivalent parts of the brain in different cases. M. Leuret, indeed, who has compared analogous parts of the brain in man and the lower animals, arrives, on the phrenological hypothesis, at the following, amongst other ludicrous conclusions:—that the organ of Destructiveness is larger in some herbivorous than in carnivorous animals; that the wolf ought to be more submissive than the domestic dog; that the rabbit should be more cunning, cruel, or courageous, than the wolf; that the sheep should have more intelligence than the shepherd's dog; that the ai-sloth should have more talent for constructiveness than the beaver and the burrowing rodent animals; and that the faculty of music should be manifested in the ox, sheep, and donkey, more perfectly than in the lark, nightingale, and finch." Now, should not all this strike every sensible person as being somewhat *de trop?* It is really too complete an affair to convey any idea that the statements faithfully represent what is in nature; it looks, *prima facie* at least, very much like a *case got up;* but let us examine some of the propositions contained in Mr Marshall's paper a little more particularly.

It is argued that, because the precise limits of an organ cannot be defined in the brain, therefore phrenological observations can lead to no certain conclusion. This ob-

jection has been anticipated, and answered, in an earlier chapter; it may however be repeated, that, until mechanical division of the nervous masses be shown to coincide with physiological distinction, the objection in question can have no force whatsoever. As before seen, an exactly contrary state of things obtains, so far as our present knowledge extends; witness, once more, the spinal cord. Nevertheless, the practical anatomist who is well acquainted with Gall's physiology, experiences no difficulty in pointing out, in the human brain, the convolution, or group of convolutions, associated with particular faculties. If Mr Marshall will select cases of extreme development, or of extreme deficiency of particular organs, he will find, *first*, the form of the organ, as it is delineated on the phrenological bust, sufficiently recognisable on the skull, and, *secondly*, he will find the subjacent convolutions large when the extreme development is large, and small when it is small, and this in a degree peculiar to these parts. By these means, he may *approximatively*, though not *precisely*, "assign limits to the assumed cerebral organs."

Mr Marshall states, moreover, that Gall, Vimont, and other phrenologists, have had too incomplete a knowledge of the cerebral convolutions in the lower animals, to identify equivalent parts of the brain, on the phrenological hypothesis. Let us clearly understand what process of observation these writers followed in studying the brains of the lower animals. Mr Combe, in speaking of Vimont, thus correctly describes his method. "Dr Vimont," says he, "rests his opinions on *direct observations made on the different races of animals, and not on loose analogies*. He has observed the energy of particular mental powers in individuals of each species, and compared this power with the size of particular parts of the brain in each, and by this means assigned special localities to different faculties, and special functions to different parts of the brain, in the different races. *The positions of the organs, as well as the*

size of each in relation to the others, he finds to be modified in each species."[1] It is necessary only to look into Vimont's magnificent Atlas of Comparative Phrenology to see abundant evidence of the correctness of this representation. Mr Combe, in his System, has copied his drawings of the skull of a "full grown cat," a "spaniel bitch," and a "crow," and he might have added many others, in which the same organs (Destructiveness and Secretiveness for example) are represented as situated in slightly different localities, and of different proportions to the other organs, in each animal; and drawings of their brains are given by Vimont in accordance with the skulls. This, we repeat, was accomplished by direct observations of the concomitance between particular mental manifestations and the size of particular parts of the brain in individual animals of each species. On what ground, then, either of reason or of fact, can Mr Marshall venture to assert that Vimont had too incomplete a knowledge of the cerebral "convolutions in the lower animals, to identify equivalent parts of the brain, on the phrenological hypothesis?" Evidently ignorant of Vimont's method, he imagines that he attempted to identify the equivalent parts by structural peculiarities alone. This, in the present state of knowledge, is impossible; but as M. Leuret assuredly did not follow Vimont's method, and as no other can lead to true results, let us beg of this most wise and consistent philosopher, to tell us how M. Leuret, an opponent, decidedly ignorant of the true bearings of the "phrenological hypothesis," was able to accomplish this identification? For, do not you, Mr Marshall, speak of sundry "ludicrous conclusions" at which M. Leuret arrived by aid of this (to the phrenologists) impracticable deed? How, in comparing analogous parts of the brain in man and the lower animals, did *he*

[1] System of Phrenology, vol. ii., p. 388. 5th Edition.

(proceeding phrenologically for the nonce) make out that, in so many instances, equivalent parts of the brain exhibit developments diametrically opposed to the fact reasonably to be anticipated "on the phrenological hypothesis?" What knowledge of the cerebral convolutions in man and the lower animals had M. Leuret, which was not already taught, essentially, in the writings of Gall, Spurzheim, and Vimont, more especially in any relation it may hold to the subject of phrenology? But the truth is,—the entire procedure of Leuret was erroneous. Safe inductions can never be obtained from comparisons of various species, when resemblances only are traced, the differences being overlooked and, consequently, not taken into the estimate. It is, indeed, certain, that the very best comparative anatomist cannot always determine analogous parts in different tribes of animals *from structural appearances alone;* and, especially, when there is question of an organisation so complicated as that of the cerebral convolutions. Many circumstances, on Leuret's part, display a thorough ignorance of the true physiology of the brain, and of the true method of studying it,—an ignorance so unequivocal as to render him quite unfit to apply the "phrenological hypothesis," in cases of the kind under discussion. The statements which he makes, therefore, respecting the cerebral development in various animals, are wholly valueless. The testimony of Vimont on the same points, is all in favour of the physiology rejected by Leuret; and, in this matter, the experience of the former is, for the simple reason, that he founds his results on a correct method, much more trustworthy than that of the latter.[1] In point of fact, no man pos-

[1] Dr Carpenter, in a note, p. 237 of his Human Physiology, says,—"If authority is to decide the matter, the author would certainly give the preference to M. Leuret, as a man of general eminence, and one who had a reputation to lose; whilst M. Vimont was previously unknown, and has brought himself into notoriety by his advocacy of phrenology." But Dr Carpenter should not forget that men "of general eminence" may have their prejudices, as well as smaller persons;

sessed of a philosophical understanding, would continue to dispute Vimont's statements, unless he had made observations by the *same method*, and attained to different conclusions.

and that, in such cases, the observations may be unduly biassed by the foregone conclusions. Tiedemann is "a man of general eminence," and he too made assertions, differing from the statements of Gall and the phrenologists, respecting the negro head; unluckily for his conclusion, however, he exhibited the premises, and the phrenological inference to which these led has already been seen. Still more likely was Leuret, imperfectly acquainted with phrenology, to make mistakes in so complicated an affair, relatively, as the comparative anatomy and physiology of the cerebral convolutions. That, previous to the publication of his phrenological researches, Vimont was unknown as a man of science, proves nothing, if he have since been found worthy to take this rank; indeed, as this circumstance left him *uncommitted*, without any formerly published convictions to retract or to confirm, it is much more likely that we should have, in his case, an immunity from prepossession, and, on this account, a more favourable state of mind for *impartial* observation. Not having "had a reputation to lose" was therefore a decided advantage. In fact, M. Vimont informs us (and his veracity is unquestioned), that, anterior to experience, whatever bias he had was all the other way; and that he commenced his long, laborious, and most expensive researches, in expectation of a different result from the one he obtained. And as to attaining notoriety by advocating phrenology,—who can doubt that so vulgar an ambition would have received infinitely more *pabulum*, had he given to the world the same body of facts in opposition to the doctrine to an advocacy of which he was only led by evidence on the subject which actually disappointed his original anticipations?

But, indeed, the whole of this argument is an unworthy mode of dealing with the question, which is an affair, not of authority, but of *fact*. Why discuss the qualifications and the fidelity of Leuret and of Vimont, when the subjects of observation themselves can, without difficulty, be appealed to? If comparative anatomy had been a sealed book to all but a very few, it would have been exceedingly proper to *reason* concerning the validity of the statements of the physiologists referred to, and to have been influenced, in some measure, by the circumstances under which the researches had been made; for, in that case, the credibility of their assertions could have been settled in no other way. As the matter stands, however, each man of science should judge for himself, from his own observation; and it is wholly unphilosophical to balance testimony on points that can be ascertained by independent inquiry.

There is, in Dr Carpenter's mention of *gaining notoriety by advocating phrenology*, an ungraciousness of tone, involving gratuitous depreciation of Vimont, to which it seems right to refer. If phrenology be not untrue

The next objection runs as follows:—"So, too, the gradual development of the posterior lobe of the brain in higher mammalia, and its extreme proportionate size in man, are unfavourable to the idea entertained by phrenologists, that it is the seat of animal instincts, which are acknowledged to be so powerfully manifested in creatures low in the scale, and which are by no means strongly marked in the character of man!" This is a very old story, and rests altogether upon the erroneous assumption that *relative position of structure*, in different species, determines the *identity of function*; and thus because, in most of the lower animals, the cerebellum is uncovered by the posterior lobe, whilst in man it is covered, it is inferred that, in the latter case, the relative development of this portion of the brain is so much the more considerable.

Now, in the first place, it should be remembered that, excepting in animals of the same kind, relative position and outward form constitute no sure guides to identification of analogous structures; in different species, organs whose functions are essentially similar will be situated variously, and in many cases they present very opposite forms. Compare the gills of fishes with the lungs of mammals: in each instance the essential function is the same, but the form and position of the organs are different; a modification being found to exist, correspondent with the differences observable in the whole economy of their being. Again, we see how nervous structures, remarkably analogous, are modified in their outward relations, in creatures widely different in elemental type. Who, from mere anatomy, could

(and Dr Carpenter constantly deprecates the idea of being set down as its opponent), it is the *physiology of the brain*,—a branch of natural science second to no other; and whoever evinces high attainments in the department becomes *ipso facto* a man of science and a philosopher, even if he have done nothing extraordinary besides. So, if Vimont's Comparative Phrenology be essentially sound, he *is* a great scientific writer, and deservedly a "man of eminence." To deny him a high position, before venturing a decision affecting the real value of his researches, is a palpable absurdity.

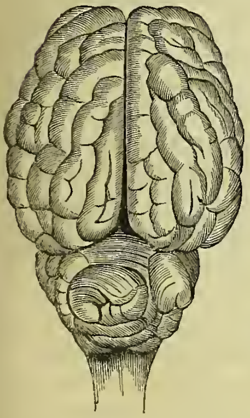
pronounce the supra-œsophageal ganglia in certain of the invertebrata, to form the analogue of the encephalon in the higher animals? Or, who can be surprised that anatomists, ignorant of the true physiology, should have believed the ganglionic chain, as found in the articulata, to be the analogue of the great sympathetic system in vertebrated animals? When species are nearly allied in type, there is comparatively little divergence in the leading characters of the organisation; still, some variety in all instances of diversity is more or less to be looked for; and, in these circumstances, we have the key to a solution of the difficulty which many persons have raised concerning the posterior lobe of brain in man and animals.

When anatomists set forth that the posterior lobe is relatively small in the lower animals, and found this assertion upon the fact, that the hemispheres of the brain do not extend backwards so as to overlap the cerebellum in the same way that they do in man, they should have some regard to the circumstance of their bodies being in the horizontal position, which demands such an accommodation of the cranium as shall fit them for it; the *foramen magnum* of the occipital bone is unavoidably placed further back in these creatures, than it is in the human subject; and this modification causes the cerebellum, of course, to be placed rather behind than under the cerebrum. A moment's reflection upon this matter will render it clear enough. The actual size, however, of the posterior lobe of brain cannot be affected by the mere circumstance of its relative position. There is also another point for consideration, in connexion with this subject:—What is to determine the limit of the posterior lobe, anteriorly? how is it to be settled where the posterior lobe ends, and where the middle begins? In estimating the development of any division of the cerebrum, it surely cannot be just to regard but one of its boundaries. The fact is,—in discussing such questions as these physiologically, we gain no advantage by the

simple aid of mechanical data; we must go to *function*, in order to make out correlative parts. In this view of the case, it must be remembered that the brain of man possesses several portions which the inferior animals want, correspondently with the possession of other and more exalted faculties of mind. It must be recollected, also, that these superadded structures are constituted of certain convolutions situated, for the most part, in the superior region of the cerebrum, just above and behind the anterior lobe; the necessary consequence of which state of things is, to supply the true posterior lobe with a comparatively backward locality.

Let any one take up the brain of a sheep, and compare it with that of man; he will, in the latter case, notice seve-

SHEEP'S BRAIN.



HUMAN BRAIN.

ral transverse convolutions superiorly, which are completely absent in the former. If the analogy of the respective structures be made a matter of any study, it may be seen that certain longitudinal convolutions, which in the human

brain belong to what is called the posterior lobe, in the sheep, run into what some may be pleased to designate the middle lobe; and this circumstance arises from the absence of parts in the one instance, which are found to be present in the other. It should always be kept in mind, that the anatomical divisions into middle and posterior lobe are quite arbitrary, and, as ordinarily formed, have no physiological basis. The author has referred to the sheep's brain as the subject for comparison with the human structure, not because it constitutes any exceptional case, but on account of its ready accessibility. As may be gathered from many of the statements that have preceded, a like principle pervades the brain and nervous system throughout the whole scale of animal organisation.

As supplying an excellent illustration of the principle in question, and as tending to elucidate many of the preceding remarks, the following account is subjoined of the cerebral convolutions of the chetah; it is from the pen of the late Mr Henry Haley Holm, whose premature decease the author regrets to have seen announced but very recently.

On the 10th September 1833, Professor Owen communicated to the Zoological Society, a paper "On the Anatomy of the Chetah," with a note from Mr Holm, expounding his ideas on the cerebral anatomy of this creature. Professor Owen, after giving his own account of the brain, prefaces Mr Holm's descriptions as follows:

"Of the constancy of the disposition of the convolutions represented by Gall and Spurzheim as characteristic of the brain of the feline genus, I was first assured by our fellow-member H. H. Holm, Esq., lecturer on Phrenology, whose attention has long been directed to this part of anatomy.

"The following note contains Mr Holm's opinions of the functions of the different convolutions in the brain of the chetah, after a comparison of it with the human brain. and that of some other animals.

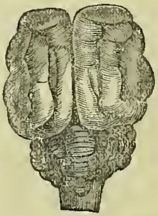
" ' In the human brain the convolutions of the posterior lobe appear formed in three longitudinal masses, meeting behind, and diverging in their progress forwards.

The internal mass,	contains—Philoprogenitiveness,	Inhabitiveness, Self-Esteem.
The middle mass,		Adhesiveness, Love of Approbation.
The external mass,		Combativeness, Destructiveness, Alimentiveness.

These masses have very frequent interconnexions, are much convoluted in their course, and have great numbers of subconvolutions.

" ' In the common *cat* we see the same type prevails, but the masses are simple. The internal mass dilates anteriorly, and forms a large portion of the anterior lobe: the middle one turns outwardly, and joins particularly the external lateral mass, which does not extend farther forwards than about two-thirds of the whole extent of the *cerebrum:* the external or lateral mass is subdivided by two transverse perpendicular fissures into three convolutions, of which, probably, the posterior may be Combativeness, the middle Destructiveness, the anterior Secretiveness and Alimentiveness; these three all unite below.

BRAIN OF A CAT, from Dr Vimont.



" ' The under surface of the anterior lobe is divided by a fissure extending nearly in the direction of the outer margin of the olfactory nerve, as in *man*, in whom the mesial convolution contains Individuality: this in the cat may perhaps include other organs.

" ' The brains of the whole genus *Felis* are similar as to these general divisions, though the convolutions vary as to their relative proportions in each species, and frequently in individuals of the same species.

" ' In comparing the genus *Felis* with the *dog* tribes, the posterior internal longitudinal mass is much smaller

than the middle; and, in the *jackal*, the middle mass is half as much more voluminous as the internal mass; while in most of the *cats* these parts are nearly equal, and in some the internal preponderates. The posterior division of the external lateral mass (Combativeness) is smaller than the middle one (Destructiveness) in the *cats*; while the opposite fact appears in *dogs*. In this respect, the *lion* approaches more to the *dog* tribe than any of the genus *Felis*.—H. H. H.'" (Zoolog. Soc. Trans. vol. i. p. 135.)

The preceding account exhibits the probable arrangements of the cerebral convolutions, in their relation to function, in the animals described; it shows that structures which, in the human brain, are located almost altogether in the posterior lobe so called, may, in the brains of inferior creatures, advance far anteriorly, owing to the absence of parts which are present in the highest type. The above description, however, in so far as it is founded on mere structural analogies, does not possess a true scientific value. It affords a reflected light only on the subject, and can at best be regarded but as probably correct in great part. The practicability of identifying particular convolutions in the lower animals, by their structural appearances alone, is by no means complete; and, under all circumstances, we must either resort to Dr Vimont's method of direct study of manifestations and development in individuals of each species, or remain ignorant of the scientific relations of the brains of the inferior animals to that of man.

Dr Vimont's work, and Mr Holm's communication, however, expose the folly of such assertions as those which Mr Marshall repeats,—that the phrenologists have made no study of the cerebral convolutions in the lower animals. In all the descriptions of these structures which Leuret, Todd, and Bowman, and others, have of late years published, it is not only not pretended that they compared manifestations and development in the lower animals and arrived at new conclusions, but it is impossible to recognise any

thing which is essentially new, which was not previously known, in principle at least, to anatomical phrenologists. Although it is nearly thirteen years since Mr Holm, through Professor Owen, communicated to the world but one item of his practical knowledge of such matters, it is known that he had previously learnt a great deal upon the subject from Dr Spurzheim, whose resident pupil he had been both in London and Paris. Mr Holm himself prosecuted comparative cerebral anatomy, in further elucidation of Gall's physiology, with great ardour. He was a Fellow of the Zoological Society; and, residing near the Society's menageries, he had ready access to the collection, of which he availed himself, to *study the habits and dispositions of the animals;* and, having permission to examine the crania and brains of those which died, he had singular facilities for comparing these habits and dispositions with the development of their brains,—in other words, for independent research. It is to be regretted that he did not publish the results which he obtained, to a greater extent; ill health, it is to be presumed, restricted his activity in this point of view.

But, returning from what may be deemed to be, in some respects, a digression, let us resume the particular question of the posterior lobe, and its relative size in man and animals. In more detailed illustration of the argument employed by the present writer, the following passage is transcribed from Mr Combe's System of Phrenology:[1]— "Besides the organs and faculties already spoken of, common to him (man) and the brutes, he is endowed with a variety of sentiments, which constitute the peculiarly human character. Of these the lower animals appear to be destitute. The convolutions which form the organs of Veneration, Hope, and Conscientiousness, in the human brain, run transversely; and in the brains of the lower

[1] Vol. i. p. 381.

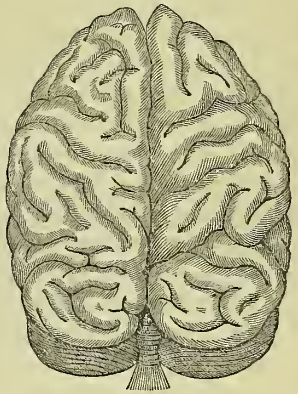
animals, so far as I have observed, no corresponding convolutions appear."

It may be said that all this is very well, if it be real; that if structures in the human brain, recognisable by their outward forms, do really exist in its more central and superior region, and which are not found in the inferior animals, it does explain in some measure the backward position of the true posterior lobe in relation to the cerebellum. But then Mr Marshall—or rather those whose objections this gentleman repeats—may say, Prove your case; let it be clearly shown that the transverse convolutions mentioned by Mr Combe actually exist. The cuts of the brain, before given on pages 317 and 319, speak for themselves, so far as they go. The paper read before the University College Medical Society, doubtless, disproves every thing of the kind. There is, indeed, a passage referring to these said transverse convolutions, and see how it runs:—" The observations of Leuret on the cerebral convolutions in mammiferous animals have led him to conclude, that there are certain fundamental gyri which first appear in the animal series; that these have a general longitudinal direction, and that in the brains of different animals they vary in regard to number, complexity, degree of subdivision, and mode of junction. In the elephant first, but afterwards in monkeys, another system of convolutions appear at the upper part of the brain, which are more or less *transverse* in their direction, and are thus easily distinguishable from the fundamental longitudinal gyri observed lower in the scale. Continuing his comparison, Leuret finds that *in the human fœtus these transverse gyri* approach in character to those observed in the monkey; and, in the adult, they reach their highest, and, indeed, an extraordinary development, occupying a considerable space on the upper and convex part of the cerebral hemispheres."

The loose wording of this passage may suggest to some persons that, anatomically at least, the higher types

of brain in mammiferous animals possess, fundamentally, the same convolutions as the human brain, differing only in degree of development. Whatever may be the actual opinion of Leuret, the combined study of cerebral anatomy and cerebral physiology leads undoubtedly to a contrary inference. Dr Spurzheim, who paid much attention to this matter, states, in his "Appendix to the Anatomy of the Brain," published in 1830, that "the brain of the ourang-outang, which, among all other brains, has the greatest analogy with the human brain in its healthy state, is yet deprived of several parts. M. Tiedemann mentions, that the brain of the ourang-outang essentially differs from that of man,—1*st*, by its whole mass being smaller; and, 2*dly*, by its smaller number of convolutions and anfractuosities; yet he had no idea of the special portions being more defective than in man, and of certain parts being wanting altogether. . . . The greatest difference is evident about the portion of the head which corresponds to the fontanelle in children: farther, about the portion under the upper lateral part of the frontal bone, and in the anterior lobes, particularly along the superciliary ridge, and in the upper part of the forehead." In Dr Spurzheim's paper, from which this extract is taken, these statements are supported by drawings, taken under his own immediate inspection, from nature; and, whatever doubt may obtain respecting their accuracy in all the details, it is certain that they are substantially correct.

BRAIN OF OURANG OUTANG.



Now, after this, the author of the paper will surely con-

fess to there being some correspondence between Gall's physiology and comparative anatomy, and concede that the objection concerning the posterior cerebral lobe, drawn from the fact of the cerebellum being uncovered by it in the lower animals, is futile, seeing that the facts which he cites not only are in strict accordance with phrenology, but, at the same time, explain away the particular objection in question. Let us see what Mr Marshall has to say upon this point. "Now," says he, "the position of these superadded convolutions, which appear to be *peculiarly developed in the human brain*, certainly does not coincide with that allotted by phrenologists to the higher mental faculties." And yet, certainly, no phrenological anatomist could have described more clearly the existence of some of the principal convolutions allotted to the higher psychical attributes; which convolutions, he informs us, reach, in the adult man, "their highest, and, indeed, an extraordinary development, *occupying a considerable space on the upper and convex part of the cerebral hemispheres*." Does Mr Combe himself, in a preceding extract, put the case more phrenologically? Shall it be said with justice, that unduly harsh criticism was employed, when it was affirmed that such writers as the author of the University College paper *are utterly unacquainted with their subject?*

Comparative anatomy, like vivisections, has been especially adduced in opposition to the assignment of the sexual instinct to the cerebellum: the facilities which exist for observing this organ, naturally make it a matter of very considerable attention, alike by the advocates and the opponents of Gall's physiology. The arguments from this source, in opposition, are very clearly put by Dr Carpenter, in the tone of whose statements and remarks there is a general fairness and candour: on this account the writer shall enter into some examination of their value. The following passages comprehend the chief objections cited by Dr Carpenter, and are taken from his Human Physiology.[1]

[1] Page 218, *et seq.*

"The results of fair observation as to the comparative size of the Cerebellum in different animals, can scarcely be regarded as otherwise than very unfavourable to the doctrine in question. In the greatest number of fishes, it is well known that no sexual congress takes place; the seminal fluid being merely effused, like any other excretion, into the surrounding water; and being thus brought into accidental contact with the ova, of which a large proportion are never fertilised. On the other hand, in many reptiles the sexual instinct appears extremely strong; and this is especially the case in the frog, the whole system of which is endowed, at the season of fertility, with an extraordinary degree of excitability, analogous to a morbid condition that sometimes presents itself in the human being. It has been remarked that, if the head of a male frog be cut off, during the congress (which lasts for some time), his embrace will not be relaxed, and will even continue, until the body of the female is becoming gangrenous from the pressure; thus showing that the action is one of a purely reflex character. Now, on comparing the size of the cerebellum of the frog with that of the cod (we exclude the higher cartilaginous fishes, in which the reproductive function has a more elevated type), we find that it is not above one-half the proportional size. Moreover, not only is the size much inferior, but the structure is much less complicated in the former than in the latter. Again, in comparing the gallinaceous birds, which are polygamous, with the Raptorial and Insessorial tribes, which live in pairs, we find that the former, instead of having a larger cerebellum, have one of inferior size. Further, on looking at the mammalia, the same disproportion may be noticed. A friend, who kept some kangaroos in his garden, informed the author that they were the most salacious animals he ever saw; yet their cerebellum is one of the smallest to be found in the class."

Now, with respect to the statements set forth in the above quotation, they may all be strictly true, and yet Gall's

physiology of the cerebellum remain unaffected. The fundamental principle guiding the phrenologists in their investigations,—the one by which they demand the accuracy of their doctrine to be tested,—and the principle by the application of which the various details have been established to their own satisfaction,—is, not that size of organ absolutely, but *cæteris paribus*, is a measure of functional power. The conditions qualifying the influence of development are constantly overlooked; so well informed a writer as Dr Carpenter does not keep this circumstance in view; he observes that phrenologists, treating of the cerebellum in connexion with the function, "assert that they can judge of its intensity, by the degree of development of the organ." On this supposition, he goes on, consistently enough, to controvert the phrenological doctrine in the manner already before the reader. It must yet be reiterated, that if Gall's proposition upon this subject is to be controverted successfully, it must be shown that, in individuals of the same species, sexual feeling is as intense with small as with large cerebella. It will not do to take instances for mutual comparison from different kinds of animals; it should be made apparent that, in such examples as the frog, one with a moderate cerebellum is more salacious than another with a large one. Frogs must be compared with frogs, and not with fishes, when *proofs* are in question. Comparisons of different species may often be very usefully made for illustration, and, in a general way, for corroboration, of the influence of organic size upon functional strength; but they are essentially unfitted for the exact determination of individual propositions. Animals differ so much among themselves in elemental type, that it is always unsafe, when the object is to infer function from development, to reason from one of a high to one of a low type, or *vice versa*.

But, in point of fact, is the state of things, in all respects, as set forth by Dr Carpenter? We do not gather from

his work that, in this matter, he has been guided by his own observations; he appears rather to have adopted the statements of Serres, made in his "Comparative Anatomy of the Brain." The present writer has not that practical acquaintance with the details of this question, which would authorise his controverting, of his own knowledge, the assertions of M. Serres, relative to the size of the cerebellum in different animals; yet certain circumstances lead him to distrust this authority altogether, on doubtful points affecting the development of parts of the brain and nervous system, for the following reasons.

M. Serres has always been one of the most strenuous opponents of Gall and his physiology; and not only so, but he has been complained of as a most unfair one. Dr Spurzheim, in the preface to his Anatomy of the Brain, observes:—"M. Serres, whose memoir was deemed worthy of its prize by the Academy of Sciences of Paris, in the first volume of his work uses our names no fewer than fifteen times, in connexion with a *single idea*, which he fancies he can refute; and generally, along with every fact that looks unfavourable to our opinion, he names us, but he always forgets to cite us in relation to very many fundamental conceptions which we had announced at the same time." Again, M. Serres, in various parts of his work on the Brain, evidences a singular fondness for generalisations affecting comparative magnitudes, which, in many cases, he most axiomatically propounds, even when no grounds whatever exist for the propositions. As an illustration of this circumstance, the following extract from Spurzheim's Anatomy of the Brain is subjoined:—

"'The spinal marrow and the corpora quadrigemina,' says M. Serres in another place, 'are so rigorously developed in the ratio of each other, that the size of the first being given in any class, or in any of its families, the volume of the latter may be determined with precision.'"

"But," says Dr Spurzheim, "the bigeminal tubercles

in the carp are much larger, in proportion to the spinal cord, than in the eel and the roach. M. Serres himself has given representations of the optic apparatus in the cassowary, ostrich, and other birds, much larger in proportion to the spinal cord, than it is in the many mammiferous animals whose brains he has figured; and the disproportion between the development of the quadrigeminal bodies and spinal cord, is even greater in the dolphins and porpoises, than in the ox, camel, and horse."[1]

As an additional example of the faulty determination of relative development, on the part of M. Serres, another passage from Dr Spurzheim is presented to the reader, taken from the same work as the preceding extract.

"M. Serres maintains that the corpus callosum is proportionate to the annular protuberance, and that the hemispheres of the brain are developed in the direct ratio of those of the cerebellum. It is easy to demonstrate this error. The masses mentioned vary extremely, and are never developed directly in the ratio of each other, neither in different kinds, nor in different individuals of the same kind of animals. To be convinced of this truth, it is enough to glance over the tables of the comparative sizes of the brain and cerebellum, which various authors (among the number, M. Cuvier) have drawn up. The proportion between the brain and cerebellum even varies in the same individual at different periods of life."[1]

Under all these circumstances, the author maintains his right to distrust the authority of M. Serres on questions affecting in any way the integrity of Gall's physiology, more especially when the opposition rests upon comparisons of development; for his haste in generalisation must have been rendered sufficiently apparent by the statements that have preceded. Whether Spurzheim be, or be not, altogether accurate in the instances which he adduces in confutation of Serres, it is quite certain that the propositions

[1] Pp. 86, 87. [2] Page 183.

cannot be held good for a moment. No anatomist of repute will confirm the assertion, that a definite proportion exists between the development of the spinal cord and that of the quadrigeminal bodies, or that the brain and the cerebellum have any fixed relation in size one to the other, or that a correspondence of this kind obtains between the annular protuberance and the corpus callosum.

However, assuming the rigid accuracy of the various facts cited by Dr Carpenter in opposition to Gall's physiology of the cerebellum, it has often appeared to the present writer that inferences are at times deduced from them which the premises do not warrant. Dr Carpenter lays great stress upon the particular case of the frog; and this instance shall be taken as an illustration of what the author means. That this reptile possesses a diminutive cerebellum is undisputed; but is it quite so sure that its sexual feeling is strong? Let us sift this matter a little more closely.

It must be premised that all phrenological observations go to show, not that every vital function concerned in propagation depends immediately upon the cerebellum, but that the *feeling* which prompts to the generative act is organically dependent upon this structure. If we analyse, so to speak, the process in question, we shall see that several functions are involved,—sentient, reflex, and secernent. The first, which involves consciousness, must have an encephalic organ; this, according to phrenology, is the cerebellum; the next —the reflex—must take place through the agency of the appropriate segment of the true spinal cord; and the last occurs through the external glandular organs. Dr Carpenter himself admits the distinct share which the reflex functions of the cord have, in the circumstances under consideration; there is every reason to believe that not only the *ejaculatio seminis*, but to some extent also the *embrace*, is under this influence, which operates independently of the will, from local excitation. "That this irritation," says Dr Carpenter, "need *not* amount to a

sensation, is proved. . . . . It has been shown by experiment, that section of the spinal cord in the lumbar region does not prevent the act from being performed, the lower division only being concerned in the reflexion of the impression." Now, it is only in accordance with analogy to anticipate that, in different species, the energy with which individual functions share in the generative process, will vary according to the different circumstances of particular creatures; and thus, that in some the act of propagation will be predominantly sentient; in others, mainly reflex; and, in some again, chiefly secretory or vegetative. We will now apply this reasoning to the case of the frog, with a view to determine the principal agency by which generation occurs in this reptile, and the correspondence of the fact with the physiology of Gall.

Dr Carpenter, having stated that in many reptiles. the generative instinct is very powerful, observes,—"This is especially the case in the frog, the whole system of which is endowed, at the season of fertility, with an extraordinary degree of excitability." May not this excitability, however, result rather from some peculiar state of the excito-motory function and the spinal cord, than from exalted *sensibility* as placed by phrenologists in the cerebellum? This would appear to be demonstrably the case, if the fact be as stated by Dr Carpenter, "That, if the head of a male frog be cut off, during the congress (which lasts for some time), his embrace will not be relaxed, and will even continue, until the body of the female is becoming gangrenous from the pressure." Dr Carpenter would thus appear to take the same position as the present writer, to an extent, indeed, beyond which he himself would deem it to be defensible, for he says "the action is one of a purely reflex character." Now, although it seems likely that the action is *mainly* reflex, it is rather in advance of the premises to infer that it is "*purely*" so. That the *phrenic* excitability of the frog at the season of fertility is but low. may be

presumed also from the long continuance of the congress, because, so far as our positive knowledge extends, an opposite state of things is incompatible with tediousness in the process. Besides, if *sensation*—a powerful excitation of the cerebellum—prevailed, would not speedy exhaustion be likely to ensue? But sensible fatigue rarely, if ever, follows the action of the true spinal nerves. And thus the just—the true physiological—explanation of the whole affair would appear to be as follows: That, in generation, the frog has but little phrenic—truly sentient—impulse, a fact which harmonises *pro tanto* with the diminutive cerebellum; that, as if in compensation, the vital impression upon the true spinal cord leads to vigorous reflex action, which continues for an unusually long period, without very obvious fatigue, as in the case of other functions of chiefly an excito-motory character. This view of the subject corresponds with the anatomy; for, proportionately to the predominance which the reflex office of the cord maintains over true animal feeling, the development of this structure would seem to exceed that of the cerebellum. As Dr Todd and Mr Bowman observe, after stating that in frogs this latter organ is very small, it is "especially so, relatively to the spinal cord." The author believes that, if this analytical mode of regarding the generative process were prosecuted extensively in comparative physiology, it would lead to some very interesting results.

Comparative anatomy has largely been appealed to by physiologists, in support of the conclusions deduced by Flourens from vivisections respecting the function of the cerebellum. Almost all the opponents of Gall's doctrine, in attempting the disproof, by comparative anatomy, of the sexual functions of the cerebellum, strive to maintain its muscular claims by facts obtained from the same source. Dr Carpenter thus sums up the evidence, furnished by the inferior creation, in support of Flourens' views, which, it has been before stated, he himself adopts.

"In the first place, the proportional development of the cerebellum is seen to be smallest in the vermiform fishes, which approach most nearly to the invertebrata; but it is much greater in the higher fishes than it is in reptiles. If we consider in what particular, that may be reasonably supposed to have a connexion with this organ, the former surpass the latter, we should at once be struck with their superiority in activity and *variety* of movement. Passing on to birds, we remark that the average dimensions of the cerebellum greatly surpass those of the organ in reptiles; but that they do not exceed those occasionally met with in fishes. The greatest size is not found in those species, which approach most nearly to the mammalia in general conformation, such as the ostrich; but in those of most active and varied powers of flight. Lastly, on ascending the scale of mammiferous animals, we cannot but be struck with the rapid advance in the proportional size of the cerebellum, that we observe, as we rise from the lowest, which are surpassed in this respect by many birds, towards man, in whom it attains a development which appears enormous, even when contrasted with that of the quadrumana."[1]

Now, all the facts may be as set forth in the above quotation, and yet they will not authorise any physiological inference affecting particular structures. The objections that apply to the whole procedure have already been sufficiently dwelt upon; it may yet be added that, in ascending from fishes and reptiles through various classes of birds and mammals up to man, there is nothing remarkable in the circumstance that a more extensive and complicated development of cerebellum should be found coincidently with greater *variety* in the movements, because there is a progressive advance in the development of the aggregate organisation, more particularly in the cerebral hemispheres, the sensori-volitional nerves, and the muscular system; and this circumstance furnishes a sufficient

[1] Human Physiology, p. 214.

explanation of the matter, without assuming the existence of any presiding influence in the cerebellum over muscular equilibrium. The reader will remember the position of Dr Prichard, that, " if we really attach any importance to such a system of correspondences," we must, in consistency, acknowledge some relation between the development of the cerebellum and the intellectual powers of man and brutes. One thing is quite certain,—whatever be the share in generation which the glandular and spinal structures possess in different animals, the higher we ascend in the scale, the more *phrenic*, in a general way, becomes the character of the whole function.

The subject of the present chapter involves so many facts and considerations, that any thing like a complete exposition of the existing state of knowledge with respect to it, would have been utterly beyond the author's capability; and, indeed, to have done full justice to this topic would have required an acquaintance with comparative anatomy and natural history, much exceeding any that he has the happiness to possess. His intention in the whole scheme of the work, has been rather to *indicate principles* fitted for the prosecution of the subject, than to collect an adequate body of evidence for exhibiting the full measure of proof which attaches, not only to the great truths of phrenology, but also to its leading details. In correspondence with this design, as relating to the harmony of Gall's doctrine and comparative anatomy, the author conceives himself to have shewn, in the first instance, that numerous facts exist which display very strikingly the harmony in question; and, in the next place, that none of the ordinary objections to phrenology, taken from the inferior creatures, will justly apply; that, in some instances, such objections come from misapprehension of the actual facts; and that in certain others, most probably, they arise from an erroneous interpretation being affixed to phenomena, not in themselves inaccurately observed.

CHAPTER X.

HARMONY OF GALL'S PHYSIOLOGY WITH PATHOLOGICAL PHENOMENA, AND THE GENERAL FACTS OF MEDICAL SCIENCE.

It has been the object of the preceding chapters to show that, although collateral sources of information, like comparative anatomy and the records of encephalic mutilations, are essentially unfitted for leading to sure or uniform conclusions, when primarily appealed to in the search for the physiology of particular portions of the brain, they are yet valuable auxiliaries in the corroboration and extension of some inference, already established by direct and legitimate induction. It is the intention, in the present chapter, to investigate the bearings of pathological phenomena upon cerebral physiology, in order to see whether, like the subjects recently discussed, they do not likewise furnish subsidiary testimony to the truth of phrenology, by virtue of the admirable harmony which they display between it and themselves.

A great deal of what has been advanced in an earlier chapter, wherein it was exhibited how morbid alterations of structure noticeable after death are but little calculated to demonstrate the actual function of the nervous masses, will to some extent be applicable in the progress of the ensuing discussion. As it has been already seen, that, in determining the harmony subsisting between the physiology and the pathology of any of these structures, we must not expect always to find visible changes after death correspondent with previous aberration in functional mani-

festation, the best proceeding, probably, by which the relations of Gall's physiology to morbid states of the encephalon can be exhibited, is an exposition of the mode in which phrenology sheds light upon the subject of insanity—of the way in which, by its aid, this class of diseases can be regarded, both pathologically and therapeutically, in the same point of view, and be studied through the same kind of evidence, as that which obtains in the instance of general medical science. And if it can be shown that, anterior to the discovery of Gall, all was uncertain and dark in reference to insanity; and that, since the application of this discovery to the consideration of what has been called mental disease, a remarkable elucidation of its phenomena has resulted, a strong support from analogy—additional secondary proofs—will have been gained to the soundness of phrenological science.

Gall's method of observation having proved—what proviously was but a well-grounded hypothesis—that the brain is the organ of the mind, it follows that a morbid condition of this structure should tend to the depraved manifestation of the mental phenomena. Phrenology, moreover, having shown that the encephalic mass is a congeries of organs—the office of each being to display an individual faculty of the mind—has afforded a flood of light to illumine the once obscure path pursued in the investigation of mental derangement; and, indeed, it bids fair, if but once generally understood and assiduously cultivated, to dissipate much of the intricacy and mystery that formerly involved this subject of inquiry—for so many ages the opprobrium, not only of medical men, but of the civilised world at large.

But here it may rightly be asked, In what manner has the subject of insanity been placed in a more advantageous position, as a matter of philosophical investigation, by the discovery of phrenology? Precisely in this way:—by enabling the student to prosecute his inquiries on very

much the same principles as those upon which other scientific pursuits are in these days based; and by enabling the medical philosopher, and the medical practitioner, to regard the matter substantially in the point of view that he regards the morbid manifestation of any of the more exclusively corporeal functions; and by enabling the inquirer to deduce principles of treatment in the management of the insane, grounded upon the analogies furnished by the actual condition of medical science—principles which, for a very long period, were most ignorantly and wofully disregarded in the case of the brain.

By demonstrating that diseased manifestation of the mental functions is associated with corresponding derangement of the material organ; by affording a reasonable and practical, if not perfectly accurate, analysis of the human mind, in an enumeration of most of its primitive powers and inclinations; by exhibiting the organic connexion between special parts of the brain and determinate mental faculties; by showing that the phenomena of insanity must be regarded as pathological conditions of the brain—phrenology (employing this term in its widest acceptation) offers for the first time a distinct clue to a system of cerebral pathology and cerebral medicine, as experimental in its nature, and as rational, as that which is afforded by other branches of the healing art.

The grand points always kept in view, and the great objects proposed, in the study of disease, by the scientific inquirer, are,—to know well the ordinary and normal state of the functions, and the organic conditions upon which this depends; to recognise accurately any deviation from such state, with its kind and extent; to ascertain, so far as may be practicable, the state also of the particular organ, or organs, whose functions are disturbed; and, with philosophical caution, to attempt the deduction of a connexion between this disturbance and the associated structural change. In the prosecution of these topics of medical

inquiry, the best attainable understanding should always exist of the relation between the corporeal organs and the agents by which they are influenced; and the conditions, in this respect, must receive attention in each particular subject of investigation. It is under such circumstances that the study of medicine becomes a philosophical pursuit, in opposition to the empirical procedure of administering medicaments in the mere expectation of a beneficial result, without reference to the known adaptation of the remedial agency to that alteration in structure, or function, upon which disease depends.

Now, phrenology furnishes the materials for a like process of investigation into the phenomena of mental derangement, concerning the real nature of which the most vague and indefinite notions prevailed, up to a very recent period,—a circumstance, however, that can excite no surprise, as all sound pathology must necessarily be preceded by a just physiology; and, in the absence of this in the instance of the brain, it was not possible that any accurate or consistent knowledge upon the subject of insanity should be had. Indeed, from the days of Hippocrates downwards, it has been discussed by a succession of writers, formally and incidentally, and yet, up to the present century even, without any very useful contribution to its history, or improvements in its modes of management,—a result that may readily be traced to imperfection in the data upon which the inquiries and the theories had proceeded. Those comparatively modern writers, who, with the advantage of a generally improved condition of pathological science, display the greatest lack of precision, and most imperfect records of the phenomena, have had no guidance, directly or *indirectly*,[1] from the physiology of Gall. The definitions and clas-

[1] It is said *indirectly*, because, at the present time, considerable appropriation of strictly phrenological ideas takes place without acknowledgment; and, in some instances, probably without a knowledge of their actual source.

sifications of various kinds of insanity, adopted by systematic writers, may be taken as evidence of the state of information upon the subject where the advantages of phrenology have not been possessed. As illustrating what is here set forth, a *definition* of insanity shall be cited, as given by an author named Harper, who wrote upon the subject towards the close of the last century, and who is quoted with great respect by the celebrated Pinel:—It is writ, "I will take upon me to define aud pronounce the proximate cause and specifie existence of insanity, to be a positive immediate discord in the intrinsic motions and operations of the mental faculty, exerted above the healthful equilibrium, its exact seat to be in the prime movement, and its precise extent just as far as the nervous power conveys its influence." As a specimen of *classification*, the author would refer to the one proposed by Dr Mason Good in his Study of Medicine, wherein this nosologist attempts to establish six genera, fifteen species, and twenty-seven varieties, of insanity; of which classification, that very able and practically useful writer, Dr A. Combe, most justly observes, in his excellent work on this subject: "Most of them are symptoms not peculiar to oue form of disease, but common to many, and depending, not on different kinds of affections, but chiefly on the particular part of the brain which is in fault; and, in short, they are symptoms which may change into others, or even disappear entirely, and yet the disease remain active and unchanged." [1]

The present writer is not aware that the above statements constitute any exaggerated or distorted illustration of the way in which mental disorder was for ages regarded. It was a vain attempt to define morbid manifestation of the mental faculties before the character of the healthful manifestation was rightly understood; and, moreover, all speculations concerning the physical conditions

[1] On Mental Derangement, p. 85.

of insanity—the basis of true cerebral pathology—were necessarily futile, in the absence of correct knowledge regarding the organic connexion subsisting between the faculties of the mind and the structure of the brain. Under such circumstances, no other result could have been anticipated.

After what has been advanced, it becomes a very simple matter, to demonstrate the harmony which exists between the physiology of Gall, and the ascertained principles of pathological science. Thus, when it is allowed that the brain is the organ of the mind, that distinct parts subserve distinct functions, that healthful manifestation of a faculty depends upon the healthful condition of the corporeal instrument, and that vigour of function is in proportion with the size, *cæteris paribus*, of the cerebral organ,—a standard becomes established by which to estimate the character and the extent of the morbid states, grouped under the designation of insanity. Phrenology, having shown that the mind, in this life, acts through and dependently upon the cerebral organisation, leads most unequivocally to the conclusion, accordantly with all analogy, that mental derangement is *functional disease of the brain*, and, so far as we can reason philosophically upon such a subject, not a disease of the immaterial soul. And, as the variety and the character of the functions of the brain have now to a great extent been developed, its pathological states may be studied upon the same principles as those which guide the scientific inquirer in general pathology. Knowing the ordinary and natural manifestations of the individual faculties, and, in great part, the organic condition upon which these depend, we are enabled to observe with greater accuracy the deviation from such manifestations, with its kind and extent; and, by diligent prosecution of the subject, we may expect to be enabled, in many cases, to ascertain, with reasonable exactness, the state of the organ whose function is disturbed; and so may attempt, without rashness, to deduce

the connexion between altered function and change in structure.

In the history of insanity, there are few points in connexion with it that have presented greater difficulties than that of *monomania*,—a term, as is known to all, used to designate those cases where the derangement of mind is upon one subject only, or, if upon several, holding a relation, in chief part, to some one particular faculty or mental quality. It must immediately strike every one who has the slightest acquaintance with phrenology, how remarkable is the harmony subsisting between this latter, and the phenomena involved in monomania. "A very important source of evidence," says Dr Carpenter, "is that afforded between the several kinds of monomania, and the forms of the brain of the persons exhibiting them; and the number of those who, having studied this question, have given in their adhesion to the phrenological system, is one of the most weighty evidences of its containing much truth."[1] The organs, in the aggregate constituting the encephalon, may, in the most perfect accordance with all that is known of disease, be in a morbid condition individually, and then each will affect only the corresponding faculty of the mind, while the others remain in a state of ordinary sanity; but, in this isolation of morbid manifestation, the derangement *need not* involve particularly the organs most largely developed, although experience has revealed the fact that such is very often the case. Indeed, if nothing had been heard of monomania, the phrenologist (being at the same time a pathologist) would have declared, *à priori*, that such a condition must at times occur, unless the brain supplied, in this respect, an exception to the general laws of the animal economy.

An objection often made may here be started, to the effect that the morbid anatomy of insanity does not furnish

[1] Human Physiology, p. 238.

that corroborative evidence of the soundness of Gall's physiology of the brain, which general physiology receives from the same source; and, in support of this objection, some eminent names may be cited. It has no validity, however; for it is a fact that, not only phrenologists, but writers ignorant of, or opposed to, phrenology, have recorded numerous cases, clearly associating mental derangement with evident change in cerebral structure; and many instances even, where the marks of disease, discovered after death, have accorded with the phrenological locality of the function previously disturbed. Still, it may be said that cases of insanity have often been observed, in which no corresponding morbid change in the brain could be detected. Undoubtedly, this is the fact. But then, let us ask, how stands the matter as regards the general principles of pathological science? Why, it is well known, that aberration in function is not *always* succeeded by appreciable change of structure, and that marked change of structure is not at all times preceded by obvious aberration in function. These propositions have been abundantly discussed and illustrated in an earlier portion of this work. Moreover, it should be recollected that the nervous tissue, in particular, is of such a character, that in some circumstances its physical changes are of a less notable kind than those of structure generally. As a matter of fact, it is notorious that there are several affections allowed by pathologists to depend upon some lesion of the nervous substance, for which no visible change in the tissue will account; such instances being technically denominated *functional*. It is very likely, however, that few examples exist, wherein functional disease would not, in progressing, finally induce such visible ravages in the structure, as to reveal the organic change, readily enough, on post mortem inspection; but then functional disease of the brain, does not always advance beyond a certain point so as to become, what, for distinction's sake, is called

organic. Yet, it may be said,—surely when an insane person dies, there is an extreme case; and you ought, under such circumstances, to find appreciable alteration in the brain, upon inspection, if your doctrine be true. If every insane person died from the immediate effects of his insanity, there would be plausibility, at least, in this position; but then such is not the case, for the insane frequently die from other diseases when the cerebral affection has not extended beyond its functional stage; and, under such circumstances, no characteristic appearances could reasonably be anticipated. But it must here be advanced that, with respect to the state of evidence concerning morbid anatomy in this class of diseases, much uncertainty exists; because, if phrenology be the true physiology of the brain, the records made of the pathology and morbid anatomy, by persons ignorant of it, can only be received with the greatest distrust, even where there is no suspicion of inaccuracy resulting from previous bias; and the fact is, phrenologists alone are competent to prosecute this department of inquiry with the full measure of success. But, in reference once more to the objection itself,—if Gall's physiology is to be rejected, because it is *imperfectly supported* (not contravened) by morbid anatomy, all physiology, for reasons and facts already adduced, ought to experience the same fate.

To enter upon any discussion regarding the causes or the symptoms of particular forms of mental derangement, or to suggest, phrenologically, specific modes of management, would be irrelevant to the present purpose; the object being simply to display the applicability of Gall's physiology to the pathology of the brain, in the same way as the principles of general physiology may be, and are, applied to the prosecution of ordinary medical science. This analogy shall be traced a little further; and it shall be shown that, by the aid of phrenology, we may adopt strictly rational systems of treatment, accordant, in all

respects, with those which we follow in the general practice of medicine.

The principle has already been asserted, that for the satisfactory treatment of all disease, the relation between the corporeal organs and the agents by which their functions are influenced, must be understood. Every organ of the body has some special relation to certain objects, or conditions, by and upon which it acts; thus, the digestive organs are influenced by, and exercise themselves upon, the various kinds of aliment; the heart and arteries are acted upon by the blood, and re-act upon it; the lungs are in the same relation to the atmospheric air; and so on. Suppose, then, that debility of any of the functions has been induced by some fault on the part of the related objects—by vitiation in quality, or by a too abundant or too defective supply,—plainly, the main indication, under such circumstances, is to modify, correspondently, the condition of the patient with respect to them; suppose, for example, the stomach to have become seriously disordered by errors in diet, a knowledge of the relation between kind and quantity of food and the stomach, enables the practitioner to relieve the latter by modifying the amount and quality of the former. Again, if an individual shall have sustained damage in the respiratory functions, ascertained to be dependent upon the inhalation of impure air, a knowledge of the relation subsisting between the condition of the atmosphere and the lungs, causes us to prescribe change of air as an essential proceeding in the management of such a case. Further, experience has taught us, that a certain relation obtains between various organs of the body and certain drugs, and a knowledge of this relation leads to an employment of the latter, when some peculiar state of the former seems to require their appropriate action upon the functions. Thus, it being ascertained that preparations of mercury, in suitable doses, stimulate the action of the liver, and influence

the secretion of bile, a right understanding of the relation of the medicament to the organ leads to an employment of the former in some conditions of the latter.

These illustrations of the leading principles which guide the scientific practitioner in the treatment of ordinary disease, will render the harmony subsisting between phrenology and pathology still more clear, in a point of view now to be discussed.

The brain, as before observed, is a congeries of organs, each one performing a special function; the locality of many of the cerebral organs and their respective offices have been made out; and, to a very great extent, we can determine the objects or conditions in relation to which the inward faculties become exercised. Thus, the faculties of *Individuality* and *Eventuality*—which include the power to know and to remember substantive existences, and events —have their related objects in the external world, and in its mutations; among the sentiments, a sense of *justice* is implanted within us, the circumstances related to which are the rights and feelings of our fellow-men; and, amongst the propensities, *amativeness* may be adduced, existing in relation to the opposite sex. In like manner, all psychical powers and qualities, manifesting themselves through the instrumentality of cerebral organisation, hold a definite relation to external circumstances, either of a moral or of a physical character, just as the lungs hold a definite relation to the atmosphere, the stomach to the food, or the heart and blood-vessels to the blood. And as in our management of disease generally, we can very often accommodate, in great measure, the relative circumstances to the disturbance of function, in such a way as to diminish the irregularity of the latter, we can also, with a knowledge of the actual fault in *cerebral* function, modify and control, in many cases, the quality of the appropriate stimulus. As an example, let us suppose a monomania to have been caused by an undue devotion to metaphysical reading, as

happened some time ago within the experience of the author, who was consulted by a youth who had received the impression, from perusing a work by David Hume, that he and all the world had no real existence; a species of mental irregularity, occurring under such circumstances, indicating to the phrenologist a state of disease mainly in the organ of *Causality*. What, in a case like this, should form the principal means of cure, upon general pathological principles? Why, obviously, to allow the morbid portion of brain to be for a time as much at rest as possible, by withdrawal of all excitation arising from purely speculative disquisitions, and by augmentation of influences related to other faculties, and so to *derive* the nervous energy from the diseased to healthy organs. And the same principle could be remembered and acted upon, more or less, in most other cases. *Moral* treatment of this character might occasionally be conceived, and put into practice, without the guidance of Gall's physiology; but even if it were, it could be in subservience to no thoroughly understood principle, and could therefore never rest upon any sure foundation.

In the *physical* treatment, moreover, of partial cerebral disorder, there is every reason for thinking that phrenology is susceptible of some distinct application. It is by no means an uncommon circumstance to discover physical signs accompanying mental derangement, in the region of the head corresponding with the organ whose function is disturbed; such signs, for instance, as increased heat, pain, and partial baldness, all of which are, more or less, within the experience of the author. In such a state of things, he cannot doubt, judging from what he has himself witnessed, that local treatment may *occasionally* be adopted with advantage. In cases where the cerebral affection would seem to be of an *acute* character, the topical application of leeches, or of cold, is at least *indicated;* and direct results, of a beneficial nature, have been verified in the

writer's own practice. It may be supposed, however, that the advantageous effects of such proceeding have come rather from the *general* effects of the treatment, than from any immediate influence exerted upon the particular cerebral part through the local appliance; and in some cases, undoubtedly, such an explanation would be the true one; but there are others in which it could not very well be admitted—cases where relief has followed treatment directly topical, which had yet resisted the same treatment on being employed at some little distance from the affected organ. As an example of this circumstance, the author shall relate the subjoined case.

Mr D. C., a gentleman about the middle period of life, of a sanguine complexion, had for several years been subject to occasional attacks of cerebral plethora, evidenced, however, in no other way than by the induction of symptoms purely subjective, which were always relieved by cupping in the posterior region of the neck. In the year 1839 he became a widower, and, from his general character and other circumstances, there is every reason to believe that he maintained this position in a manner consistent with the strictest morality: the cerebellum, however, was largely developed; and, under such circumstances, it occasioned the writer no surprise to find that, within twelve months from the death of his first wife, this gentleman became the husband of a second. Very shortly after this second marriage, he called upon the author with the following exposition of circumstances. He set forth that his general health was good, that he had of late been little, if at all, affected with any general sense of fulness in the head; but that, during *coitus*, he had been suddenly seized, on successive occasions, with an acute pain at the back of the head, which progressively increasing, abruptly interrupted the consummation of the marital rite. With his hand he indicated the region of the cerebellum as the locality of this distressing symptom. He went on to state,

that feeling a natural reluctance to make mention of these circumstanees, he had had recourse, on his own responsibility, to the remedy which had always previously relieved the general affection of the head; but although the cupping in the neck had been liberally employed, it had failed to procure any sensible relief. On being consulted in such a case, it was impossible that the author should not fall back upon Gall's physiology. Indeed, he made the diagnosis, and deduced, moreover, the indications of treatment, by its aid. He directed that the posterior region of the head should be shaved, and a number of leeches be applied as nearly as possible in a line corresponding with the transverse diameter of the cerebellum; and after this should have been done, that the patient should make use of a cold lotion in the same region. The negative prescription may be inferred. These directions were carefully attended to and obeyed. In a very few days the threatening (shall it be called?) of cerebellar apoplexy, arising from only the normal and legitimate excitation of the organ, had totally disappeared. The author has heard of no recurrence of the affection, although he has been in the constant habit of seeing the patient in question.

Corresponding cases, in reference to other parts of the encephalon, occur from time to time; and many of these have been recorded, especially certain paroxysmal affections of the organ of Destructiveness. It must yet be reiterated, that no such facts constitute *direct* proof of phrenology. They serve very well for subsidiary evidence of its truth; but, in their very nature, they can do no more.

The writer has heard it advanced, that, in undue cerebral excitation of a partial nature, no benefit should be expected to ensue from local blood-letting, because direct vascular communication does not exist between a particular portion of brain and the corresponding region of the scalp. This objection seems plausible at first sight, but it

will not endure a close examination. The *sympathy of contiguity* abundantly explains the phenomenon ; and, however difficult it may be to give a reason for the fact of parts contiguous to one another exerting reciprocal influence (apparently from the mere circumstance of such contiguity). the fact itself is certain. Irritation of the lining membrane of the stomach is often relieved by leeches applied to the epigastrium ; yet, between the abdominal integuments and the actual stomach, there is as little direct communication of any kind as between the scalp and the cerebral substance ; and other parallel instances will immediately occur to the reader.

Where no physical signs of local disease of the brain evince themselves, and yet where the function of some special part is obviously deranged, it would seem not improbable that physical treatment directed to the external region of the organ should, in some cases, conduce to a beneficial result. Upon this point, however, the author wishes to speak with caution, as he has not *seen* the success of such practice. Still, phrenology being true, it is reasonable to expect advantage under these circumstances; a favourable result would be in perfect harmony with what is known of practical medicine in other departments. For example, there are some forms of dyspepsia, where no tenderness on pressing the epigastrium is apparent, or other local sign beyond the irregularity of function, and yet where decided relief has followed the application of leeches, or counter-irritation.

A large proportion of the cases met with amongst the inmates of a lunatic asylum being of an incurable nature, it may be well to refer to the assistance which phrenology renders, in some instances, to the medical attendant in his discrimination of the hopeless from the other patients. Many who have displayed mental imperfection from their birth, owe this misfortune, proximately, to faulty size or configuration of the head. It were almost superfluous to

observe, that, as phrenology would readily lead to the discovery of such instances, it would enable the practitioner to give in these cases a much more firm and accurate prognosis, than it would under other circumstances be practicable to give; just as an *asthmatic chest* (in popular phrase), dependent upon organic malformation or deficiency of size, would at once be pronounced incurable.

Before closing with the question of harmony between the pathological applications of Gall's physiology and the general science of medicine, some recurrence must be made to the subject of the cerebellum. The opinion regarding the balancing function of this organ has been maintained also by the aid of pathology, as well as by vivisections and comparative anatomy. On this account, it may be well to determine, so far at least as is practicable, the just value of certain pathological phenomena, which have been appealed to as corroborative of the theory of Flourens.

There have been many cases related in which tumours, and other morbid changes, in and near to the cerebellum, have been detected after death, and in which the patient had evinced, during life, a deficiency in the control ordinarily possessed over the voluntary muscles; and such facts are constantly referred to, as supporting the idea concerning the muscular function of the organ. Nay, the medical periodicals, within the last two or three years, have repeatedly published accounts, wherein the cerebellum has been mentioned in connexion with morbid phenomena, as if its balancing function were an established and generally recognised fact! Some lesion is found in the structure; the patient, in life, had been unsteady in movement—the comments set forth, very likely, that the morbid alteration is accounted for by the previous derangement in the function of the cerebellum,—evinced by reeling and staggering; just as if such symptoms, to a greater or less extent, did not arise in most other affections of the encephalon, especially in those occurring at the base, and

near to the medulla oblongata. Much that is applicable in this stage of the argument has already been advanced in preceding chapters. It may be sufficient to repeat here, that contiguity of the cerebellum to the motor strands of the spinal cord, satisfactorily explains all the lesions of motion that at any time follow its diseases. Disease of the cerebellum is often succeeded by loss of vision: how do pathologists, most commonly, account for this circumstance? By contiguity of the structure primarily disordered to the central ganglia of vision—the optic tubercles. It would be just as reasonable to infer that the cerebellum is the organ of sight, because blindness frequently follows its lesions, as to conclude that it is the central seat of a power co-ordinative of muscular action, on account of irregularity in this particular sometimes being exhibited in its morbid states.

The views entertained by the present writer on the class of diseases often adduced in support of the doctrine of Flourens, are so fully illustrated and borne out by the following case, that it shall be cited *in extenso;* it is one of softening of the right lobe of the cerebellum, and is from the *Clinique Medicale* of M. Andral.

" A seamstress, thirty-one years old, who had hitherto enjoyed good health, about six weeks before entering the hospital of La Charité, had a fright whilst menstruating: the menses were suppressed, and immediately after their disappearance, she was *seized with dizziness and an acute pain in the back part of the head, towards the right side.* The dizziness disappeared after a bleeding, but the pain of head remained; it was unconnected with any other symptom for eight days; subsequently the patient began *to experience an annoying sense of formication at the ends of the fingers of the left hand; she could use this hand but awkwardly, and was astonished at seeing what she handled with it fall continually from her grasp:* she soon became unable to work with it at all; the entire arm seemed very heavy. After some time,

the lower extremity of the left side became weaker, and in about a month the patient had *complete hemiplegia of the left side.* At the same time that the patient thus lost the power of motion of one of the sides of the body, *her sight*, till then extremely good, *became very weak*, and five weeks after the invasion of the first symptoms, she *became completely blind.*

" This was the state in which we first saw her : deprived of sight, and of the power of moving the limbs of the left side ; the pain of head had then become less acute ; the patient, however, still felt it, and referred it to the lower part of the occipital region of the right side.

" The paralysed limbs were flacid, and could be moved in all directions ; the skin covering them still retained its sensibility ; no trace of paralysis of the face ; the pupils, moderately dilated, still contracted on the sudden approach of light ; the appearance of the eyes was natural ; there was, however, all but complete blindness, the patient being scarcely able to distinguish day from night ; the intellect was perfect, the pulse natural ; the catamenia had not reappeared since they were suppressed by the fright. Leeches were first applied to the nape of the neck, then to the genital organs ; aloetic pills were frequently given, and subsequently the back of the head was covered with a blister.

" No change appeared for the first three weeks of her stay in the hospital ; then, without any known cause, the *pain of head suddenly became more violent, and extended to the entire cranium ; the extremities of the left side, which till then had remained entirely immovable, were several times agitated with convulsive movements*, which were slight in the lower extremity, but very violent and almost continual in the upper limb ; acute pains accompanied these convulsions ; the *intelligence soon became disturbed ; complete delirium* set in ; for twenty-four hours the patient spoke, and was agitated incessantly ; she then fell into a profound coma. in which state she died.

"*Post-mortem examination.* The pia mater extending over the convexity of the cerebral hemispheres was very much injected, as was also that covering the cerebellum. The substance of the brain, properly so called, was marked with a considerable number of red points, and presented no other lesion; lateral ventricles distended with a great quantity of limpid serum; the fornix and septum lucidum natural. Externally the cerebellum appeared healthy; but we had scarcely removed some layers of the substance of its right hemisphere, proceeding from above downwards, when we found an immense cavity, where this substance, deprived of its normal consistence, was changed into a greyish *bouillie;* this softening occupied at least two-thirds of the right hemisphere of the cerebellum; it partly *implicated the prolongations which go from the cerebellum, either to the spinal marrow, or to the tubercula quadrigemina, or to the annular protuberance:* it did not extend as far as the lower surface; in no part of its extent was there either injection or infiltration."[1]

The passages rendered in italics are those to which the author, in commenting upon the above particulars, directs special attention. The case exhibits, in the first place, obvious signs of congestion in the posterior region of the encephalon, attributable most likely to the sudden suppression of the catamenia; the post-mortem inspection places it beyond a doubt that this revulsion was to the cerebellum. Concerning the state of the sexual feeling, nothing is recorded; but, for obvious reasons, this defect in the evidence must nearly always exist in the case of grown-up females, unless they be out of their senses. But, suppose such lesions to weaken, or to paralyse altogether, the function in question, the same effect ensues upon almost any severe disease, so that such a fact, if verified in disease of the cerebellum, would amount to nothing. To resume.

[1] Spillan's Translation, p. 202.

however,—the patient, after some time, began to experience irregularity both of motor power and of cutaneous sensibility; these symptoms, Flourens would explain by his muscular theory of the cerebellum, and Foville probably by his idea of its being the centre of common sensation; the contiguity of the cerebellum to the medulla oblongata and upper segment of the spinal cord, and its fibrous communication with these structures, account, rationally enough, for both sets of phenomena, without involving the views either of Flourens or of Foville. As the disease progressed, considerable paralysis of motion ensued, doubtless from augmentation of the disorder in the direction of the cord; coincidently herewith, vision became lost, in all probability from extension of the disturbance in the other direction, towards the corpora quadrigemina, through the *processus e cerebello ad testes.* Shortly before death, morbid manifestation of the functions of nearly all the nervous centres took place. A reference to the appearances noticed after the patient's decease will justify the pathological interpretation put upon the phenomena by the writer; and, indeed, the whole case displays, in every respect, harmony, and certainly no discordance, with the physiology of Gall. Yet such are the facts ordinarily adduced by opponents, in pathological confirmation of another physiology of the cerebellum than that of the phrenologists.

Another case of disease of the cerebellum, of the same general character as the preceding, shall be cited; it allows of very much the same explanation, as to its collateral bearings; it was read before the Reading Pathological Society, by Dr Cowan, and published in the Provincial Medical and Surgical Journal, for April 16th, 1845.

"Mrs C., aged 44, mother of sixteen children, brown hair and eyes, sanguine temperament, robust health, active habits, and possessed of an unusually calm and energetic mind, suffered, in January 1844, from attacks of bilious headache, which quickly yielded to ordinary treatment.

"In February, she underwent great anxiety and fatigue in consequence of the alarming illness of a daughter, and was observed to be excited, restless, and to lose her flesh and strength. The following month she occasionally awoke with severe headaches, which gradually subsided after getting up, and was also sensible of peculiar, uneasy, quivering, fidgetty SENSATIONS in the legs, particularly the left; these, at times, forced her to continual movements for relief. She felt as if she could not sit still.

"In May, she complained of partial NUMBNESS in the left cheek, and was now observed to STAGGER in her walking; deviating from a straight line, and to the left, as if she had lost the *directing* power. It was compared by those about her to the effect of slight intoxication. The headaches became more intense, at times agonising, with distressing vomiting; and on the 8th of June, when preparing to ride out, she was seized with præcordial pain and palpitation, violent congestion of the head and face, embarrassed speech, and mental confusion. The day following, the POWER OF UTTERANCE WAS IMPAIRED, THE LOSS OF SENSATION IN THE CHEEK COMPLETE, AND SHE WAS DEAF IN THE LEFT EAR. For these symptoms she was bled, leeched, and salivated, and blisters and a seton applied.

"For a month subsequently the headaches materially diminished, but the excitement, restlessness, distressing feelings, uncertain movements, emaciation, and general lassitude, gradually increased; and for two or three months she was liable to fits of hysterical laughing, excited by the most trivial cause.

"In August, her deglutition was rather difficult, and strabismus, principally of the left eye, was noticed, with PARTIAL LOSS OF VISION ON THE SAME SIDE. The headaches returned with still greater intensity, and were referred to the left parietal and occipital regions. The irregularity in her voluntary movements gradually affected the arms as well as the legs. She required to be held and directed

in every act, supported at times by two assistants, and pushed forwards by a third, to enable her to move about the room; and if, by accident, she fell when attempting any effort alone, she was quite unable to raise or to assist herself. There was no paralysis of movement or sensation to the last, nor was one side more distinctly implicated than the other; but the *controlling*, *the co-ordinating power of the muscular system*, appeared abolished, or nearly so.

"For six or eight weeks before death, which took place on February 18, 1845, she was merely removed from the bed to the sofa; the debility and emaciation became extreme, and the sphincter muscles ceased to act.

"Her mental condition, from the first, was characterised by excitement and great restlessness, seeking relief in change of scene and position; an inability to be quiet; a certain indescribable distress; an effort to restrain and conceal what she felt to be at variance with her natural habits and feelings; a childishness of thought, with great fickleness of memory and attention; and yet, throughout a long and painful illness, supported with unvarying patience, and no ordinary fortitude, there were intervals of unexpected and remarkable revivals of the natural feelings and mental powers, with a capability of utterance strikingly contrasting with her ordinary state. This singular waking up of the mind occurred within a few days of her death. She appeared sensible to the last.

"*Post-mortem*, thirty-six hours after death.—Extreme emaciation; scalp dry, thin, and bloodless; skull of average thickness and density; it separated easily from the dura mater, and the inner table was deeply furrowed, and of a rather porous structure. All the membranes presented great venous turgescence, with soft dark coagula in the larger trunks; about an ounce of pink-coloured serum drained from different surfaces to the base; a small quantity of opalescent fluid was lodged under the arachnoid in

the sulci; the membranes were of healthy firmness and transparency; no bony irregularities.

"The cerebral convolutions were deep and numerous; the grey matter very distinct; and the brain throughout was vascular, particularly the cerebellum.

"At the base we found a dark-red, spongy, and highly vascular substance, presenting all the well-known characters of encephaloid carcinoma, incorporated with the anterior extremity of the left lobe of the cerebellum, of which it seemed a prolongation, and passing forward in inseparable connexion with the pons, and FOLLOWING THE EMERGING FIBRES OF THE CORRESPONDING CRUS CEREBRI, IT TERMINATED ABOUT AN INCH FURTHER, without penetrating the ventricle, in the medullary substance of the middle lobe, where it presented a more broken-down and disorganised appearance. It originated about a quarter of an inch posterior to the pyramidal fissure, involved the whole thickness of this portion of the cerebellum, and nearly an inch in width, penetrating iregularly into the substance of the pons and left crus, from which, as well as from the cerebellum, it seemed to grow and sprout. The pia mater only covered it at a short distance from the cerebellum, and the general surface was irregular and shaggy. THE LINE OF SEPARATION BETWEEN THE DISEASED AND HEALTHY STRUCTURES WAS INDISTINCT, but no membrane intervened, nor was there more than very slight softening of the surrounding cerebral pulp. THE MEDULLA OBLONGATA, AND THE PONS, WERE PUSHED TO THE RIGHT, FORMING AN OBTUSE ANGLE WITH THE SPINAL MARROW, AND THE LEFT PYRAMIDAL AND OLIVARY BODIES APPEARED STRETCHED AND SLIGHTLY SOFTENED. The left pneumogastric nerve was thinner and more filamentous than its fellow, and the seventh, fifth, fourth, and sixth nerves, passed over and partially through the morbid growth, and, at their exit from the skull, were vascular and soft.

"At a precisely corresponding point of the right side, in the centre of the white fibres, as they leave the cere-

bellum to pass over the pons, a small nucleus of encephaloid matter, about the size of a large pea, was imbedded, as if formed at the expense, and not by the displacement, of the surrounding tissue.

"The morbid growth on the left side, was, therefore, limited by the tentorium superiorly, by the petrous portion of the temporal bone anteriorly, and could only increase in bulk by pressing the mesocephale upwards and to the right."

The present writer entirely agrees with Dr Cowan in thinking, as expressed by himself in the "*Remarks*" subjoined to the above detail, that "The correspondence between the lesions, discovered after death, and the symptoms during life, is, in the instance now before us, more direct than we are often enabled to establish;" and he has rendered such passages in more prominent type, as appear to himself to be calculated to exhibit the correspondence in question. The extensive disease at the base of the brain accounts alike for the paralysis of some of the senses, indistinctness of utterance, the staggering gait, and imperfect control over the muscles of the extremities. The cerebellum was seriously implicated, and (could the fact have been ascertained) the sexual feeling was probably weakened. Dr Cowan says,—"Whatever other functions the cerebellum may discharge, it is now generally admitted, from the experiments of Flourens, Magendie, and many others, that the integrity of this organ is essential to the proper association or harmonising of the voluntary movements. The loss of this power formed the most striking peculiarity of the case we have narrated." The italics, moreover, in preceding account, are Dr Cowan's own; referring, specially, to the loss of the *co-ordinative* power over the muscles. From these circumstances, it is to be inferred, that Dr Cowan has given in his adhesion to the doctrine of Flourens; and that, too, on the evidence exclusively of vivisections and pathological phenomena. Better things were to have been expected from such a quarter. When

Dr Cowan alluded to the experiments of Flourens and Magendie, had he forgotten that the two were in antagonism one of the other, both in the matter of fact and of inference? And as to the loss of the *directing* influence over the muscles, and the staggering gait, how could it have been otherwise, when, besides the general disease at the base of the brain, " the medulla oblongata, and the pons, were pushed to the right, forming an obtuse angle with the spinal marrow, and the left *pyramidal* and olivary bodies appeared stretched and slightly softened?" How is it that, in this instance, Dr Cowan should have fallen into the vague jargon of the day? You must not, Dr Cowan, give the respectable sanction of *your* name to those who would primarily make out the cerebal functions by vivisections and pathological observations. No one knows better than you that,—" To render vivisection available for the discovery of the function of an organ, the latter must be distinct; its boundaries and distribution known; the possibility of its being injured or destroyed without materially interfering with the other organs and functions must be ascertained; and, after its removal, we must be able clearly to determine what functions are deficient. Now, while these conditions may be fulfilled in the section of particular nerves, *they are wholly deficient in the instance of the brain, and we cannot, therefore, rationally expect to determine the cerebral functions by the means we have been considering.*"

" Pathological observations, though of greater value than the preceding, *are liable to many of the objections already stated*, and must be received with great caution as evidences for or against the functions of particular portions of the brain; but if conjoined with the *information derived from extensive physiological research, may prove powerfully* CONFIRMATORY of the latter."[1] These excellent truths, Dr Cowan, you published, yourself; and they are well calcu-

[1] Dr Cowan on the Physiology of the Brain. Prov. Medical Transactions, vol. vii. p. 391.

lated to rectify much error that is prevalent in the actual philosophy of medical men. You must permit one, who, some years ago, read what you wrote with pleasure and with profit, to return some of the obligation under which he is to you,—by reproducing, for your consideration, your own reasoning.

The association of morbid states of the cerebellum with unmistakeable signs of erotic disturbance, has many times been witnessed; and the positive facts which display such association possess a value corroborative of the phrenological doctrine, which no negative facts can weaken. If, in disease of the cerebellum, no departure from the customary demeanour be observed, in so far as the sexual feeling is concerned, there is no evidence that functional power is not *depressed:* but, if erotic manifestations evince themselves under such circumstances, there is obvious proof of *undue* excitement, because it is what a patient would naturally exert his will to conceal from medical observation. Yet, cases exemplifying the positive influence of disease of the cerebellum upon the generative instinct, have been recorded by opponents, as well as by the supporters, of Gall's physiology.

Even M. Serres collected and published a number of cases, mostly observed by himself, where erotic disturbance, of an unmistakeable character, had accompanied disease of the organ: these were cited by Dr Gall, in his own works; and, in Mr Combe's translation of "Gall on the Cerebellum," they also will be found in English. On the strength of these cases, Serres did not hesitate to affirm, that the dependence of the sexual instinct upon the cerebellum was *proved* by pathological phenomena; but, like a consistent opponent of phrenology, he restricted the function to the middle portion, or vermiform process, of the organ; doubtless conceiving that, in this way, he should nullify the value of Gall's labours, whose *peculiar* method of observation was only applicable to the aggregate cerebellum.

The following account, published in the London Medical and Surgical Journal, for June 21, 1834, is from Dr Stokes of Dublin. After allusion to the cases reported by Serres, he mentions two instances which had fallen beneath his own notice, exhibiting a connexion between disease of the cerebellum and visible excitement of the external organs.

" I have now seen two cases," he says, " in which this connexion was observed. In the case of a young man who was brought into the Meath hospital some time ago with paraplegia, it was observed that the penis was in a state of constant erection, and there were continual seminal emissions. On dissection, an effusion of blood was found in the cerebellum, and another in the hemisphere opposite the paralysed side. There was another case of a patient who was attacked with apoplexy and paralysis of one side, but with the unparalysed hand he continued to attempt the act of masturbation, so that it was necessary to tie down his hand. On dissection, there were several effusions in the substance of the cerebellum. All these facts strongly go to prove the connexion which subsists between the cerebellum and the generative function ; and I think it would not be unsafe to make the diagnosis of disease of that organ in cases of cerebral disease, where the genital system was much excited."

A great many cases of similar character, and of very decided import, may be found in the volume which includes the Translation of Gall on the Cerebellum, collected by Mr Combe from various sources. Since the publication of that work many others have been observed, and recorded, for the most part, in the medical periodicals. The combined force, indeed, of all the pathological phenomena bearing upon the phrenological doctrine of the cerebellum is so considerable, that if, in any instance, we could determine primarily as to physiology from pathology, it would certainly be in attainment of the conclusion. that this structure subserves the function of generation, in so far at least

as this depended upon any phrenic condition or impulse, and principally, because many of the facts recorded seem almost incapable of explanation upon any other theory. The following, however, occurs in Dr Carpenter's Human Physiology :—

" A case has been recently communicated to the author, in which the sexual desire, which had always been strong through life, but which had been controlled within the limits of decency, manifested itself, during a period of some months preceding death, in a most extraordinary degree: on *post mortem* examination, a tumour was found on the pons varolii. This fact harmonises with the view that the sexual instinct, if connected with the cerebellum at all, has its seat in the central lobe; but it also corresponds equally with the idea, that the medulla oblongata is its centre."[1]

But the *pons varolii*, as the great commissure of the cerebellum, is in more direct and extensive communication—by fibres, too—with its lateral portions than with the central lobe; and, as to the medulla oblongata, it must be considered, so far as evidence exists, to be functionally *preengaged.* The fact related by Dr Carpenter is in the most complete and undoubted harmony with Gall's doctrine.

Dr Alison, discussing the subject of generation, evidently regards " the mental feeling, or emotion of desire,"—the phrenic condition—as " probably connected with some portion of the cerebellum ; *because it has often been observed to exist in an unnatural degree, in cases of disease or injury of that part.*"

Dr Alison is no phrenologist; and yet, upon evidence which any philosophical phrenologist would deem to be insufficient to *prove* the functions of any encephalic structure, he is not indisposed to admit of a decidedly phrenological conclusion. So wedded are many most able and

[1] Note, p. 219.

honourable minds to habitual modes of thought! They will have no quarrel with your particular propositions, if you supersede not their fundamental philosophy; they care not for Gall's conclusions so much as they shrink from his *method*. And yet assuredly there is no other method whereby the relations subsisting between the structure and the functions of the encephalon can be validly determined,—relations which, once ascertained by a just system of investigation, will be certain to harmonise fully with every sure fact which pathology or any other collateral source may afford.

CHAPTER XI.

ON QUALITY OF STRUCTURE, AS INFLUENCING FUNCTIONAL MANIFESTATION.

Throughout the whole system of nature, the influence of size upon power is modified by the effects of quality. But the great law of size existing as a measure of power, other conditions being equal, is undisturbed by this circumstance. Whether we appeal to the organic or to the inorganic kingdoms of nature, ample confirmation will be afforded of the position just maintained. Thus, as a general rule, a large bar of iron is stronger than a small bar; yet the size, regarded alone, does not make known to us the exact degree of strength. Every one is aware that the degree of perfection with which the processes of fusion, separation of the iron from the dross, and of cooling, have been conducted, greatly determines the quality of the metal, and thereby its strength and efficiency. Let us suppose some piece of mechanism, constituted of iron, the strength of which and of its various parts we are anxious to determine,—how should we proceed? Why, having no actual experience of its powers of resistance, we should assume, if the external appearance indicated a sound and normal condition of intimate structure, that the capability and strength of the whole material were correspondent with those properties in other similar machines; and, in most cases, it might be inferred from inspection, whether the quality were excellent or otherwise. Yet, in some instances, this could not be done with precision; and hence, we could

not reach to positive certainty of the *absolute* power of resistance inherent in the machine. In an attempt, however, to ascertain the relative strength of the individual portions, the uncertainty would exist no longer; knowing that the entire structure was formed of material from the same source, which had gone through exactly the same process of elaboration, we should experience no difficulty in saying that the largest portions were the strongest.

The same rule will be found to prevail if we ascend in the scale, and exhibit a comparative illustration taken from the vegetable world. Here, although magnitude of structure forms an important element, not only in physical, but also in vital energy, it does not afford any absolute index of this endowment. Considerations of quality must always interfere. If we enter an orchard, and contemplate trees of the same species, we hesitate not to affirm that size of each tree, or of portions of the same, *cœteris paribus*, furnishes a measure of productive energy; and so, in one recognised as being of fair, uniform, soundness of texture, we could readily point out, from the size, the branches that yielded the most abundant fruits. Yet, *à priori*, we could not predicate the *absolute* productiveness of each branch; nor could we make unfailing comparisons between the capabilities, in this respect, of one tree and another. We might, however, by assuming that the objects examined presented an average state of structural quality, obtain results that, probably in a large majority of cases, would remarkably accord with the fact. Nevertheless, inasmuch as this intimate quality of material is not a matter to be accurately determined by mere inspection, we never could ascertain positively from physical signs, the *absolute* amount of functional energy in any given instance.

If we take the animal kingdom, the same principle will obtain. Size, with an equality of other conditions, is here, too, an index of vigour. Large animals, as a general rule, possess greater muscular power and energy than smaller

ones; yet such a statement, unqualified, cannot be maintained in all instances. We may indeed affirm, and truly, that, in animals of the same species, structural magnitude, with similarity of other circumstances, indicates the degree of individual power; and, in particularising somewhat further, we can also assert that the individual organs display, usually, greater energy of function when large, than when small; thus, capacious lungs, *cæteris paribus*, are more effective in the execution of their office than smaller ones; a large heart will, commonly, propel the circulating fluid with more force than one that is less so. The same principle holds good in respect of the abdominal viscera. Still, no physiologist deems volume, *absolutely*, to determine functional energy in these instances; amongst other circumstances affecting the result, *quality* of the organs is ever included,—a condition, however, not so open to observation that its influence can in all cases be thoroughly and precisely predicated, in the absence of experience of the results.

Yet a more apposite illustration of the proposition is afforded by the muscular system. No one will deny—it is too obvious to mere sense to admit of doubt—that the size of a muscle, all other conditions being equal, is a measure of its power; it cannot, however, be always determined by inspection of muscular development, what is the *absolute* strength of the individual. In many cases, very considerable muscular power is observed, without its being associated with particularly large muscles. Examples are not of unfrequent occurrence, where a man shall possess quadruple the average strength of his fellows; but, although in such instances the muscular system is always well developed, it certainly is not found to excel the customary proportion to the same extent. Every one has heard of the Greek Milo, the Samson of profane antiquity, whose strength is said to have so much surpassed that of ordinary men, that, without breathing, he could carry an ox for the space of a furlong. Had the muscles of this extraordinary man pos-

sessed, in point of magnitude, the same superiority, he would have constituted a very monster; yet we nowhere read that he was so characterised. No doubt can be entertained that all prodigies of strength that have at any time appeared have had large muscles; but, certainly, individuals are constantly seen with the highest degree of muscular development, who do not display any very prodigious powers; and, doubtless, this circumstance may be partly attributed to inferiority in the quality of structure. Differences in the motor nerves may also, in some degree, account for this; as illustrated in the great vigour of the bird's wing, with small size in the muscles but large nerves of motion, and in the less vigour of the fish's muscles with large size and smaller nerves. How, then, it may be asked, have we attained a knowledge of the law relative to the connexion between muscular size and bodily strength? If men with moderately developed muscles shall sometimes exhibit the power of those possessing a more capacious muscular system, with what certainty or security can the application of such a law be made in any particular instance? In reply, it is to be observed that the discovery of size of a muscle, *cæteris paribus*, being an index of functional vigour, has been made by the constant observation of ordinary facts; it has been seen that muscles charged with the weightiest duties in the animal economy have had a corresponding magnitude, and that small muscles occur where the required movements need but slight applications of force. Hence, in the same species very generally, and in the same individual almost invariably, a certain proportion obtains between a particular muscle and the assigned offices. Yet no one, understanding such subjects, has ever attempted to determine the exact amount of strength of individuals by notation, merely, of the muscular development; nor from such data, exclusively, to be making constant *comparisons* between the muscular energies of *all classes of persons*. At the same time, were such a rule proposed, and submit-

ted to experiment, there is no doubt that very precise results would sometimes be obtained, by an assumption, in each case, of an average accompaniment of the associated and qualifying conditions. Still, as a scientific proceeding, such an application of a branch of physiology would be objectionable, because open to fallacy. Yet, *in the same individual*, with a healthful condition of the whole system, and with a well-balanced state of habitual exercise of the particular muscles, *size would yield the indication of relative power;* and, as a principle, such a procedure, in the estimate of muscular strength, would universally apply.

So far, then, as we have advanced, the importance, not only of size, but also of intimate structure, to the manifestation of power, is clearly perceptible,—whether regarded in relation to the mere physical properties of brute matter, or to the vital endowment of organic textures. And as nature seems to delight in unity of method, and constancy in design, should we not have anticipated, even in the absence of all experience upon the subject, that a like principle would pervade the physiological constitution of the brain? Should we not have expected (the cerebral functions being *assumed*,) that not only size, but also the quality of structure, would exert an influence upon vigour of function, conformably with what appeared to be the general plan of creation? The anticipation could not have been otherwise; analogy is all on one side; and it is this alone which, in default of direct evidence, can lead to the formation of a probable inference. But, in coming to a conclusion upon this matter, we do not depend exclusively upon analogy; actual facts reveal the truth, that the law which determines the vigour and the energy of inorganic matter, of vegetable, and of general animal life, prevails, in like manner, in the particular constitution of the brain.

Now, in every attempt to determine the true value of Gall's discovery, it is of the last importance that right notions should be entertained of the influence of quality

of the brain upon the functional manifestations. This consideration, however, does not always receive from popular students of phrenology the attention which it demands—a circumstance attributable, in many cases, to defective acquaintance with the subject in a physiological point of view; and, in others, to an indisposition to exercise the proper labour and caution required in all serious investigations of the economy of nature. In the practice of a certain class of persons calling themselves phrenologists, we often witness mistakes from ignorance or neglect of the conditions affecting the quality of the cerebral organs,—mistakes which are most unfairly adduced as evidence of the unsoundness of Gall's doctrine; and, occasionally, novices in the inquiry are deterred from its study by such circumstances. Indeed, it is a disposition but too common amongst many *soi-disant* phrenologists, to pronounce absolutely upon the native energy of talents and inclinations, from an almost exclusive regard to the development of the brain, as indicated by the cranial configuration. It is within the experience of all who have devoted any considerable period to the testing of phrenological doctrines by appeals to nature, that cases from time to time occur, which occasion the greatest difficulty in the minds of recent inquirers, seeming indeed to present serious anomalies, and even contradictions to what they have been disposed to regard as well established principles. For example, individuals are observed, remarkable for their mental qualifications, and actual attainments, whose frontal development is yet inferior to some others, ranking below them in point of intellectual dignity. And again, we shall meet with persons with large foreheads, whose education has been in no respect deficient, and yet who never, in point of actual efficiency, attain more than a respectable mediocrity—who never display the talents, or obtain the distinction, of many inferior to them in cerebral development and extraneous advantages. Yet, how is all this to be explained? Are we hereby to

distrust phrenology, since the laws of every science must be of invariable application, to be valid? Must we regard the facts and observations of Gall, Spurzheim, Vimont, Combe, and others, as coincidences, rather than as illustrations of general truths? Or, shall we not rather attempt to ascertain whether sufficiently comprehensive notions are entertained concerning the cerebral physiology of Gall, when such instances as those referred to, are supposed to form anomalies or contradictions? Shall we not see whether the real principles maintained by Gall do not actually embrace such cases, so as to render unimpeachable that character for uniformity and universality which may rightly be claimed for the great laws of phrenology, as for those of every other science ascertained by observation of natural phenomena?

Whilst the fundamental principle of Gall's physiology is comprehended in the proposition that the brain is the organ of the mind, the next principle in point of importance, and one more special, is contained in the affirmation that size of the brain generally, and of its parts particularly, is an index of functional power, *all other conditions being the same.* From the terms in which this axiom is expressed, it is not difficult to perceive the source of the fallacy which too frequently vitiates the practice of persons only superficially acquainted with the subject. The definitiveness of the phraseology relating to size, as an influential element in the constitution of the brain, arrests and retains the attention too forcibly to admit of its being readily lost sight of, whilst the comparative vagueness with which other conditions are alluded to, often leaves no other impression upon the mind than the remembrance of two words, hardly understood in some cases, the Latin "*cæteris paribus*." Again, the discovery of Gall, having resulted from observation of size, in *extreme instances* of cerebral development, and appeals to the incredulous being necessarily made through the same medium, an additional reason is afforded, why the superficial student

should be puzzled and confounded when, after having given in his adhesion to the conclusions, upon the strength of such appeals, he observes, occasionally, the supposed anomalies or contradictions before referred to.

The real phrenological doctrine then, is, and has always been, that size of an organ is the measure of power, only when all other conditions are equal; and, amongst these, not the least important is the *quality* of cerebral structure. It may, however, be safely asserted that, upon the combined influence of size and quality, strength and capability depend, in so far as these properties are constitutional. "The energy of the functions of organs," says Dr Gall, "does not depend upon their development only, but also upon their excitability."[1] But here the question arises, Are we enabled to decide with certainty respecting the quality of brain, before any experience of its actual efficiency has been obtained? Do physical signs exist externally, empowering us hereupon to speak, in the same manner as we are, in most cases, prepared to speak concerning the size of an organ? In a few words—Can we, in the present state of knowledge, declare, from outward characteristics, what is the *absolute* force of innate character? In reply to this latter question, it is to be observed, that we can only do so approximately; for, although in every suitable subject of examination, we can readily estimate size of the cerebral organs, we cannot determine, *with unfailing accuracy*, the actual character of that other condition—quality, upon which natural capability so much depends.

The only approach which we possess to determinate knowledge, relating to the subject of the present discussion, consists in the doctrine of the temperaments, which has been variously regarded and taught, in different ages of the world, by different individuals. The ancients, with Hippocrates at their head, regarded the bodies of all the

[1] Sur les Fonctions du Cerveau, tom. i. p. 196.

higher classes of animals as consisting of four elements, viz. of blood, of a watery fluid, and of two kinds of bile, yellow and black; and the temperament was defined according as each of these assumed elements had the preponderance:—there was, first, the *sanguine*, produced by an undue predominance of the quantity of blood in the system; next, the *lymphatic*, dependent upon an excess of the watery fluid in the various animal tissues; third, the *bilious* or *choleric*, resulting from a surplus of the yellow bile; and, lastly, the *atrabiliary* or *melancholic*, produced by excess of the fancied elementary black bile. These respective peculiarities of temperament were considered to be associated with corresponding powers and dispositions; and thus, what phrenologists regard as the combined effect of cerebral development and quality, was attributed by many of the ancient physiologists to the temperament only. The sanguine temperament, for instance, was considered to be associated with quickness of perception, tenacity of memory, a lively and luxuriant imagination, a disposition readily roused to anger but as easily appeased, and an undue attachment to the indulgence of sense; and, in like manner, each of the other temperaments was regarded as the cause of certain other mental characteristics. Such views, in their main bearings, have continued in great favour with many physiologists up to a very recent period.

But, in point of fact, all denominations and classifications of temperament have been arbitrary and varying; and, to the present day, the greatest obscurity hangs over their origin and causes; although, in some respects, we can take note and reason concerning their effects upon functional manifestation. So far as the author remembers, Gall did not enter into any detailed investigation of this matter of the temperaments; Spurzheim, however, attempted to effect some reconciliation between the rude observations of previous physiologists upon the subject,

and those facts which had revealed the existence of distinct cerebral organs. It appeared to him to be incontestible, that the varying bodily conditions which had led to the formation of doctrines concerning temperament, did actually exercise some influence upon the mental manifestations; and he was himself led by observation and reflection to adopt a fourfold division into lymphatic, sanguine, bilious, and nervous, preferably to any other; conceiving that, to a very great extent at least, the constitutional *activity* of the cerebral organs was determined by peculiarity of temperament. Subsequent observations by other physiologists have confirmed the views of Spurzheim with regard to this matter.

The distinction between *power* and *activity*, of any human faculty, is easily appreciated. We are all aware that, in respect to the muscular system, one man is exceedingly quick, restless, and vivacious, but unfit for energetic labour; while another is little disposed to exertion, tardy in his motions, but able, when set to work, to execute feats of strength which the first individual would attempt in vain. There is a perfect analogy in this respect, between muscular power and all the other animal functions, including those of the brain and nervous system; and the difference in question is to be traced, in great part certainly, to variations in the quality of the organs, as indicated by temperament.

The physical characteristics of the *lymphatic* temperament, are a softness of the fleshy parts, from undue repletion of the adipose and areolar tissue; commonly. a fairness, though thickness, of the skin; the hair most usually of light, flaxen, or sandy complexion; a plumpness of figure, but without expression; the pulse weak and slow; and a languor and want of energy in all the vital actions. Individuals of this temperament are generally remarkable for their aversion to both mental and bodily exercise; and whatever be the development of brain in such instances.

the quality is generally so low as to render first-rate excellence impracticable in any pursuit. Persons of the lymphatic temperament, even when endowed with high mental power, will often be surpassed in their qualifications, both for the common and extraordinary duties of life, by individuals of far less native strength of mind, but who, with a more favourable temperament, and consequent love of exercise, have laid in larger mental stores. In drawing inferences, therefore, from development of the cerebral organs, great caution should be observed when the temperament is lymphatic, as sometimes the activity of powerful organs will hardly have been induced, in the absence of strong external stimuli.

The *sanguine* temperament may be distinguished by the red or light brown hair, blue eyes, and a fair florid complexion: the arteries and veins are large, and generally very superficial; the pulse full and frequent, the skin soft, somewhat thin, and rather delicate; the body largely made, and inclined, especially in the middle period of life, to obesity. This temperament, probably more than any other, is generally regarded as influencing the mental economy otherwise than by its effects upon the activity and vivacity of function; and, by some physiologists, it is even yet considered to form the bodily condition determining the psychical peculiarities enumerated in a preceding page; but an appeal to facts will readily show, that neither this, nor any other temperament, is invariably associated with any given character in such a point of view. Individuals of the sanguine temperament, however, nearly always possess brains decidedly more active than those of lymphatic subjects; a result which can but be attributed to some influence exercised by it upon the quality of structure.

The *bilious* temperament conduces to a still higher degree of functional energy in the brain than the sanguine; it is characterised by a pulse strong and hard, as in the latter, but somewhat more frequent; by prominence of the cuta-

neous veins; the complexion of a somewhat swarthy appearance; the hair black, or of a darkish brown; the body but moderately fleshy, and the muscles firm, and well marked; and often there is a peculiarly strong, and somewhat harsh, expression of the countenance. Individuals characterised by this temperament have generally a considerable share of native energy, manifesting their predominant powers and dispositions with remarkable keenness and persisting endurance. Unlike individuals of the lymphatic, or even of the sanguine temperament, they require no very powerful external stimulant to bring about vigorous display. The possession of this temperament is generally regarded as the most favourable to attainment of excellence; generally the persons who, under all circumstances, evince an indomitable activity, both of mind and body, are found to be of this temperament, being neither enticed to indolence or sensuality by the lymphatic or sanguineous constitution, nor too speedily exhausted of power, as is very often the case with individuals of the nervous temperament.

The temperament denominated *nervous* is the most favourable to mere activity of the mental powers; but the activity is not so enduring as in the case of the bilious temperament. In many instances, the mind associated with the nervous temperament may be likened to a taper burning with a light too brilliant, and thence the more speedily consumed; or to ignited flax, which astonishes by its glare, but whose flame is as transitory as it is brilliant; there being in this constitution an extreme susceptibility of excitement in the nervous system. The external characteristics are,—a soft skin; fair and thin hair; sometimes a paleness of the complexion, and sometimes a hectic tinge; small and soft muscles; delicacy of the whole organisation; generally, a slenderness of form; a sparkling vivacity of the cornea; and a quick sharp pulse. These signs, in the aggregate, constitute indications of the

nervous temperament; giving rise, as before observed, to the highest degree of cerebral activity. Individuals so characterised will be sure to be in a state of very energetic excitement, on the application of stimuli inadequate to the result with the mass of mankind. If a person have strong animal propensities, he will, unless powerfully under the influence of rightly-directed moral feeling, be almost sure to run a short but active career of profligacy and libertinism; if the intellectual organs be in relatively large proportion, he may speedily wear down his bodily strength, and sink prematurely into the grave, the victim of excessive mental exercise; or if the religious feelings predominate greatly over the intellect and the animal propensities, he may become a religious maniac; and so on. In children, the possession of the nervous temperament, under the present rage for early and strenuous mental excitation, is sometimes the worst of misfortunes, since their young brains, being so readily excited, often afford, in the mistaken judgment of their guardians, the highest evidences of genius; and thus the poor victims are goaded on, until some affection of the exhausted brain or nervous system hurries them to the close of their ill-fated career, if it do not leave them the prey of some serious nervous affection, as epilepsy, hysteria, or even downright fatuity. The author would so far digress as to exhort, seriously, every guardian of youth, or infancy, to consider well the consequences, such as have just been mentioned,—consequences which are more or less within the experience of every one engaged in the practice of medicine.

The several temperaments, as just described, are rarely found unmixed; they are generally found to run into each other. Thus, a mixture of the *sanguine* with the *bilious*, the *bilious* with the *nervous*, or the *sanguine* with the *lymphatic*, is very frequent; and, most usually, those temperaments whose characteristic activity makes the nearest approach, will be observed to be combined. This, how-

ever, is not invariably the case; for we may have a mixture of the *nervous* and *sanguine*, or of *lymphatic* and *bilious;* and, indeed, no limit can be placed to varieties of combination. In estimating the effects of a mixed temperament, the characteristic activity of each entering into the mixture should be regarded, and the mean be deduced.

Now, although indications of temperament very materially assist in the estimation of quality of brain, in so far as this leads to *activity* of functional manifestation, it is yet very uncertain to what extent actual *power* or intensity is revealed by the outward signs just detailed. There are some persons who, from want of real knowledge, conceive that development and temperament, conjointly, furnish all necessary data for pronouncing *absolutely* upon innate characteristics; but such persons have not studied phrenology in all its bearings. Indeed, amongst merely *believing* phrenologists, much misunderstanding appears to prevail upon the whole subject of the temperaments. It is frequently thought that we have the same positive knowledge upon this matter as we have respecting cerebral development; but the fact is different. Concerning the origin and causes of differences in temperament, we have little or no exact knowledge; nor do we understand in what manner they affect the quality of the organisation so as to influence its functional activity; consequently, the facts observed and recorded upon this subject are not, strictly speaking, susceptible of scientific arrangement, in such a way as to rank amongst recognised principles in physiology. Nevertheless, the facts (as facts) have a certain degree of scientific value, capable, moreover, of leading to some, though an imperfect, practical result; for the general views of temperament which Spurzheim advanced, and which Mr Combe and most other phrenologists are in the habit of acting upon, are undoubtedly realised and confirmed by observation. They show that, very often at

least, excellence of quality of brain is associated with particular indications of temperament; that functional manifestation is facilitated and rendered more active under certain of these conditions rather than under others; and there is very little doubt that, besides influencing the activity of the cerebral organs, temperament is to a certain extent a condition of power.

But notwithstanding the general accuracy of these views, it is yet certain that other conditions of structural quality exist, very sensibly influencing both power and activity of brain, which, however, are not appreciable by external physical signs. Dr Spurzheim was evidently aware of this circumstance, for although in most cases he relied upon indications of temperament as yielding the measure of activity of the cerebral functions, we see him, on other occasions, suggesting that *activity* of an organ was probably influenced by *length* of cerebral fibre, and *power* chiefly by its *thickness*,—an idea quite inconsistent with the doctrine that temperament of itself reveals every condition of quality, as influencing manifestation. Attaching all due importance to temperament, accordantly with the views of Spurzheim, Mr Combe, in discussing the question,—Do we possess any index to constitutional qualities of brain?—distinctly recognises residual phenomena. He observes: "There are some constitutional qualities which can be judged of only by knowing the qualities of the stock, or race, from which the individual under examination is descended. I have observed a certain feebleness in the brain, indicating itself by weakness of mind, without derangement, in some individuals born in India of an English father and Hindoo mother. The tinge of colour, and the form of the features, indicate this descent. I have noticed feebleness and sometimes irregularity of action in the brains of individuals, not insane, but who belonged to a family in which insanity abounded. I do not know any external physical indication of this condition. The tem-

peraments indicate, *to a certain extent*, important constitutional qualities."[1]

The present writer holds, then, that whilst the vigour of the mental faculties, as well as their activity in display, is greatly dependent upon the *quality* of their material organs, we do not possess *sure and determinate means of predicating* this quality; that, nevertheless, the bodily differences to which the idea of temperament is attached, aid in the formation of some rational estimate thereof, because, in a great majority of instances, a certain coincidence is found to obtain between the quality of the visible tissues—the skin and hair, for example—and that of the brain; that still the precise causes affecting such coincidence, are quite unascertained; and that, consequently, no doctrine of the temperaments that has yet been advanced, can rank as a sure branch of physiological *science*.

Since there are no certain physiognomical characteristics by which we can determine the quality of cerebral structure, are we, it may be asked, in a better position for pronouncing upon this matter from careful examination of the substance of the brain itself? It has long seemed to the writer to be, at least, a probable fact, that as the quantitive relations of the grey and the white matter are not uniform in different individuals, the brain which contains beyond an average proportion of the vesicular or grey substance correspondently excels in the power and intensity of its functional manifestation,—a view which rests upon the assumption that the theory is sound, which regards the vesicular structure as the source of energy. As the proportion of the grey to the white substance, maintains most probably, in the same individual, something like uniformity throughout the whole system, the author's view would explain in what manner *relative* power is revealed by development, although the *absolute* strength may remain an uncer-

[1] System of Phrenology, p. 49.

tainty. If, however, the hypothesis which attributes the paramount office to the grey nervous substance, should not ultimately be substantiated, it may then be imagined that the relative amount of areolar filamentous tissue which connects the cerebral cells and fibres together, may vary in different cases, so that the encephalon in one person shall be formed more of true cerebral substance, and less of areolar tissue, than in another. Whilst this idea would explain differences in the quality of the aggregate structure, it would likewise exhibit a state of things, in an anatomical point of view, perfectly harmonious with Gall's doctrine of cerebral development, since the largest organs in any one head would, under such circumstances, not the less display the predominant energy. Or, indeed, it may be supposed that each set of conditions determines quality of brain, as influencing functional manifestation. Of course, these observations are only thrown out for the consideration of those who have adequate opportunities for testing their validity by investigation of the facts of the case. Many facts, indeed, already recorded, corroborate the idea respecting the paramount importance of the vesicular structure in the constitution of the cerebral convolutions; in the head of Cuvier, the brain, independently of its absolute magnitude, was remarkable for the great relative amount of the grey cortical substance.

"If it be true," says Dr Carpenter, "that the grey matter only is the source of power, and that the white matter is merely a conductor, we have no right to assume that the total size of the organ affords a measure of its power, until it has been shown that the thickness of the cortical substance can be judged of, by the size of the brain, or of any part of it. Certainly there is a considerable variation in this respect among different individuals; and it is yet to be proved, that the relation is constant in different parts of the same individual brain. Until this is substantiated, all inferences drawn from correspondence between the promi-

nence of a certain part of the brain, and the intensity of a particular function, are invalid; that is, if the general doctrine of the relative functions of the grey and white matter be true."[1] This reasoning is plausible enough, but a very few remarks will dispose of it. In the first place, Dr Carpenter must once more be reminded, that it is no where maintained by phrenologists, "that the total size of the organ affords a measure of its power;" there must, in addition, be an equality in all the other conditions. In the next place, though it does "remain to be proved" that the relation of the grey to the white matter is constant in the same individual brain, yet the fact is highly *probable.* No persons have investigated the nervous system, in reference to this point, more extensively than Gall and Spurzheim; and these physiologists affirm that, in tracing the development of the whole up to and including the brain, they always found a very definite relation, in amount, between the two kinds of nervous tissue. Now, as the grey matter of the cerebral convolutions exists in determinate connexion with the several bundles of cerebral fibres, would it not appear to follow that, in the same individual, the relative quantities should be tolerably constant in different parts? It must be remembered, that the *observations* of Gall and Spurzheim concerning the quantitive relations of the two substances, are entirely independent of the *inferences* which they deduced regarding the origination of the one in the other. Lastly, Dr Carpenter asserts, that until the probable fact now under discussion be proved, "all inferences drawn from correspondence between the prominence of a certain part of the brain, and the intensity of a particular function, are invalid." Nevertheless, the truth or the falsehood of such inferences must be determined altogether by the *fact;* if the correspondence in question exists as a fact, the state of things with respect to another matter cannot alter

[1] Human Physiology, p. 237.

it. If the proportions of the grey to the white cerebral substance will not harmonise with what is already demonstrated respecting "correspondence between the prominence of a certain part of the brain, and the intensity of a particular function," the circumstance goes to prove that "the general doctrine of the relative functions of the grey and white matter" is *not* true; that is all. Although a fact can invalidate a mere inference, an inference can never discredit a demonstrable fact. In dealing with this point, Dr Carpenter, indeed, seems to be inverting the natural order of all sound reasoning, making the premises subservient to the deduction. It is only in attacking phrenology that such arguments as these would be employed by a writer of Dr Carpenter's usual accuracy of thought; the author would suggest, however, that the stock of public and professional prejudice may at length be overdrawn, and fallacies be viewed in their true light, even when directed against Dr Gall.

In the uncertainty which exists regarding some of the conditions which affect the quality of brain in its relation to functional energy, some persons, upon a superficial consideration of the subject, may think that at least the physiognomical value of Gall's physiology has been greatly overrated; and some parties may probably imagine that its evidences are materially weakened by the considerations just advanced. For these reasons, the author will subjoin a few remarks illustrating the extent to which he conceives that innate characteristics may be determined by outward physical signs. And he would premise by observing that his intention, in the present chapter, is mainly to render prominent, and to enforce, the practical truth, that, inasmuch as in the present state of knowledge we cannot always determine the intimate quality of the brain, no well instructed phrenologist *attempts* to pronounce with certainty, in every case, upon the absolute power of the cerebral organs. The true phrenologist disavows altogether the practice of com-

paring the organs in different individuals, except for the single object of accustoming the hand and eye to recognise the form and appearance of each organ in different degrees of development; because, for example, the same *absolute* size of the organ Tune may in one man of a low quality of brain be accompanied with only a relish for music, while in another of a high quality it may give rise to powers of musical performance or composition. Hence it will be obvious that any comparisons of cerebral development in different persons (as significant of the relative vigour of their natural endowments), must ever involve doubt as to accuracy in the inference; because, even in cases of identity of education and other outward circumstances, the organic quality of structure cannot altogether be determined by mere indications of temperament; and nothing but experience of the actual manifestations can, in some instances, reveal the degree of excellence in the said quality. The proceeding of the practical phrenologist in an inquiry into natural character, can only lead to exact results in the comparison of different parts of the same head, in an estimate—not of the absolute, but of the relative size of the organs. We may thus, by physiognomical signs, determine, under circumstances of health, and excluding cases of early infancy and of old age, whether in an individual the propensities, sentiments, or intellectual faculties, constitute the leading traits of character; and the proportion in which, as compared with the average of mankind, these various properties exist in the natural constitution of that individual. In further analysis, it may be ascertained whether any primitive faculty be associated with so large a development of its correspondent organ as certainly to form a prominent feature in the particular character. For example, if the organ of Destructiveness be found in much higher proportion than the organs of the other propensities, the corresponding disposition will not fail to distinguish the possessor. The same procedure may, with confidence, be

adopted in the estimate of all the leading, well-established organs. Where one or more are in obvious advance of the rest, no hesitation from ignorance either of external circumstances, or of quality of the brain, need deter the observer from at once speaking out: for, most assuredly, in all cases in which no disease exists (and this when present may generally be detected), the proper consciousness of the party under examination will not fail to confirm the judgment, whatever may have been the outward demeanour of the individual, and however he may be esteemed by his friends. It were superfluous to add that, in cases of excessive deficiency of development, corresponding results may confidently be predicated, with the same disregard of cerebral quality and other circumstances, as in the opposite class of facts. But it must be reiterated, that all such decisions should have exclusive regard to the mind of the person under examination; the judgment can never with certainty proceed beyond a declaration concerning the relative power of the faculties of the same individual. It cannot be asserted with perfect confidence, whatever be the development or the temperament in any given instance, that any one possesses extraordinary endowment, and that he will *certainly* become eminent under favourable circumstances; we never can say, and *be quite sure*, that a person is gifted with great talents, by mere examination of his head. But if we are informed that great powers do actually exist in some cases, we can tell by observing the organs most prominently developed, what are the faculties distinguishing the party, and in what direction the talents will be likely to run. The same principle, of course, must guide us in estimating the sentiments and propensities, in connexion with development of their respective organs.

In case of the intellect, however, we must not always expect to discover very large organs where we happen to notice powerful manifestations, because, in a high state of

civilisation, the circumstances of human society are such as generally subject the intellectual faculties to the concentrated influence of every possible stimulant; and, from this circumstance, it often happens that persons with only a fair development of the anterior lobe of brain, especially if coincident with high moral feeling and some ambition, will display considerable mental power. It not unfrequently happens that, after a very exalted opinion has been formed of some individual from perusal of his writings, or from some distinction that he may have achieved, the greatest disappointment is felt if the head do not in such a case present a remarkable front. There is the head of such a person, it will be said, presenting nothing that is unusual, and yet behold how able a man he is; there are the heads of A and B more favourably developed, yet assuredly they have not the talent of this person; what does phrenology say to that? To calculate results upon a principle of this kind is not the proceeding of any but the most empirical phrenologist; not only size of the cerebral organs, but all the other conditions, require to be estimated. In addition to the external circumstances affecting any individual, there is the uncertainty as to quality of structure (not fully removed by indications of temperament) and which always creates a doubt upon the question of absolute power; so that comparisons of one head with another, in his point of view, can never be safely made. As, however, with some rare exceptions, uniformity of quality prevails throughout every part of the *same* brain, development may justly be taken as indicative of the *relative* vigour of the several faculties and dispositions. Indeed, in all the instances, in allusion to which the author has heard expressions of disappointment on account of the forehead, the particular *bent* of intellectual character has always been correspondent with the development; the most powerful faculties having always been associated with the best developed organs.

It must yet be stated, that, in predicating character from cerebral development, we shall, in a very large number of instances, obtain some striking results, even if abstraction be made of all considerations relative to quality of brain. Because it happens, in regard to phrenology as to the subject of any other investigation, that a certain average state of things will obtain as the rule, extremes forming the exceptions; and so, in the intimate constitution of the brain, a degree of uniformity will exist amongst the generality of persons; and this the more particularly, under circumstances of coincidence in the various modifying causes, either of a moral or of a physical nature. Hence, where the sphere of observation on the part of an inquirer is but very limited,—where the immediate relatives and friends, for example, alone supply the subjects for experiment, it sometimes happens that the highest and most exclusive confidence becomes placed in the indications of character furnished by the *size* of the various organs; for, under such circumstances, an identity to a great extent prevailing in all the modifying agencies, development supplies almost the sole elements of difference. And any one who shall commence phrenological observations by generalising from such restricted experience, will be very likely, in his future practice, to regard size as an absolute measure of power, to the utter neglect or oblivion of the *cœteris paribus.* When, however, an observer is not confined to some small locality, or to one uniformly constituted circle, the case will be different; instances will come perpetually under notice, where many sources of difference besides those of cerebral development have to receive attention, before a valid result can be gained. Variations, not only in the habits, associations, and scholastic education, but also in the quality of the organic structure (only partially indicated by the temperament), may constantly interfere to prevent character from being made out by development with the facility that was probably expe-

rienced in the earlier career. There is in all this, however, nothing to weaken the evidences of phrenology; the law will obtain universally that, *cæteris paribus*, the largest organs in any given head will indicate the most powerful faculties and dispositions, and consequently the *direction* in which the particular character will be likely to develope itself; and within this law may be comprehended the supposed anomalies and contradictions in phrenological experience, to which detailed reference was made in opening the subject of the present discussion.

CHAPTER XII.

ON PHRENOLOGY AS AN INDUCTIVE SCIENCE, AND AS A SCIENCE SUSCEPTIBLE OF PRACTICAL APPLICATION.

Although, at the present day, there are few individuals who will not admit that there is some truth in Phrenology, there is exhibited, in many instances, a disposition to underrate its importance, as if it were a subject unworthy of a serious and pains-taking investigation. Very often, the leading facts upon which it professes to rest, will be undisputed; and yet, when the legitimate inference is attempted to be drawn from them, they will be characterised as not of sufficient weight to be allowed to rank amongst the truths of science. This would appear to have become a species of cuckoo-note; many persons averring themselves to *believe in Phrenology* as something "very curious," yet not admitting that its scientific, physiological character should be recognised. Here, at least, there is an *apparent* anomaly; truths arrived at by observation of natural phenomena, are declared to be unfit to be regarded as science! It becomes, then, a very important and interesting inquiry to ascertain what are the marks of *scientific* truth, and to what extent these appertain to the discovery of Gall?

What do we understand by "science," and what constitute its essential characteristics? The etymology of the word simply implies "knowledge;" but the conventional employment of the term restricts its application to that KNOWLEDGE *which leads to the development of principles;* and an "inductive science" would appear to comprehend any department of inquiry wherein "principles" become gained

by direct observation of facts. In this point of view, let us look at Phrenology; let us see if its observed facts are not of such a character, and so conditioned, as fairly to authorise certain general expressions, implying that the facts involve principles, by virtue of their universality under appreciable circumstances; let us determine, in short, whether sure inductions have not rightly flowed from the phrenological observations, of a nature, too, calculated to enforce the assent of all who will *seriously* investigate their true character.

If the true indications of an inductive science have here been set forth, the next step would appear to be to particularise their application to the present subject. It may be as well, however, to inquire, first, in what way the marks of scientific truth belong to matters universally recognised as science; and then it can be more readily determined whether, by a like process, their application can be made to a disputed subject. For this comparative illustration, let us select the instances of Chemistry and Geology; the one furnishing an example of a science well matured, and the other of one in its infant condition.

All whose attention has been directed to the subject, are aware that chemistry, made up as it is of a vast variety of natural facts, affords many examples of general laws, under which may be classed most of the individual phenomena; thus, it is a law, that all the elementary substances in nature are susceptible of entering into combination with each other only in definite proportions; it is a law that all gases and vapours dilate equally on the application of equal increments of temperature; and it is a law also, that in the combustion of any substance which is incapable of flying off in fumes, an increase of weight takes place. These constitute some of the "principles" of chemistry; and they are such, because the fact which is affirmed in each proposition, is universally true. Tried, then, by the definition previously advanced, chemistry establishes its

claims to rank as an inductive science, because the "knowledge" of which it is composed, gained by observation of natural phenomena, *does* lead to the development of principles; these latter embracing the conditions under which, in the present state of evidence, universality of the facts occurs.

Geology is probably the latest of the recognised sciences, and furnishes an excellent instance for the employment of the process just adopted with reference to chemistry. Although it possesses an immense accumulation of materials obtained by observations carefully conducted, the number of general truths is comparatively small, and many of the inductions are professedly provisional. These circumstances, indicative of its imperfect condition as a science, furnish, however, no reason for its exclusion from the category. For, although the principles are but few, there are some which may be regarded as fairly established. It is, for example, a law, that the superposition of the stratified rocks occurs in a definite and uniform relation; and also, that specifically different forms of animal and vegetable life characterise the particular strata. Hence, then, if the premises be allowed, the facts collected by the geologists do not stand as isolated phenomena, but constitute, like those of chemistry, that sort of knowledge which leads to the development of principles, and so determine the scientific character of the subject to which they belong.

There was formerly a period in the history of chemistry and geology, when neither had any claims to rank amongst the inductive sciences; when, notwithstanding the immense amount of labour bestowed upon them, scarcely a solitary law could be said to have been discovered or anticipated; and when, from the erroneous mode of procedure, observation or experiment could hardly be said to *lead* to the development of any principle. It is unnecessary to dwell upon that era in the history of human research, when alchemy prevailed, and speculative accounts of the origin and progress of the earth's formation,—analogous

in so many respects to cerebral physiology anterior to the epoch of Gall; these things, however, may be alluded to as illustrating the position, that the claims of any department of inquiry to rank amongst the sciences, proceed, not from the nature of the subject itself, but from the *method of investigation* pursued, and from the *results* obtained.

Now, if it be a correct definition of inductive science to state that it is the knowledge which, gained by observation of natural phenomena, leads to the development of principles; and if chemistry and gèology have been rightly considered with reference to this definition, Phrenology clearly possesses a just claim to a position amongst the inductive sciences, provided the propositions announced as its general laws, or principles, result fairly from the evidence accumulated in their favour, and provided the facts affirmed in each axiom be universally true.

The question, then, as to whether Phrenology be, or be not, entitled to rank among the sciences, must be determined, not by vague and indefinite notions regarding its probable importance or utility, but altogether by the validity or the fallacy of the *principles* upon which it is based. Let us, therefore, proceed to inquire whether the leading facts observed and recorded by phrenologists, do not develope and support certain general laws or principles. It will have been seen, from what has been advanced in preceding chapters, that, in maintenance of the doctrine that the brain is the material instrument by which the mind or conscious principle is manifested in this life, it is asserted, that wherever, throughout animated nature, there are distinct and unequivocal signs of consciousness, the presence of a brain is observed; and, conversely, wherever a total absence of brain is induced, artificially, or by natural causes, indisputable indications of mind never exist. Now, if these facts be universally true, the principle would surely seem to result, that the brain is the organic condition upon which mental manifestations de-

pend. But let us examine this matter a little further. In collateral support of the proposition, it will have been noticed that phrenologists affirm that the conditions of the mental faculties are universally influenced by variations in the state of the brain, just as the known functions of other organs of the body are affected by their actual condition. If these facts be admitted, can it be doubted that they warrant the induction that the brain is the organ of the mind? Most assuredly, if we be certain that the function of the eye is to see, that of the stomach to digest, or that of the liver to secrete the bile, we must infer from the above evidence, that the functions of the brain are to manifest the faculties evincing consciousness. Let us here compare the proceeding by which the function of the eye is inferred with that of phrenologists regarding the office of the brain, and we shall see how far the processes are analogous. For this purpose, the author will apply the language which he has used above, to the evidence upon which the former inference rests, changing the words only where it is required by the illustration; thus it will be seen to what extent the analogy exists between the two proofs.

"Wherever, throughout animated nature, there are distinct and unequivocal signs of *vision*, the presence of an *eye* is observed; and, conversely, wherever a total absence of *eye* is induced, artificially, or by natural causes, indisputable indications of *vision* never exist. The conditions of *vision* are universally influenced by variations in the state of the *eye*."

The above paragraph comprises a summary of the evidence upon which rests our knowledge of the office of the eye; and it is quite clear that, if the facts affirmed by phrenologists be true, the conclusion which they deduce from them is inevitable, if it be an established fact that the eye is the organ of sight; for it will be observed that, in the two instances, the modes of proof are exactly similar. The first principle of Phrenology, then, as an inductive science, is, that *the*

brain is the organ of the mind; and this is a scientific truth, purely within the domain of Gall's discovery, notwithstanding that at the present time the assent to it has become general, even among parties who affect to disclaim Phrenology. Gall was the first who laboriously and earnestly interrogated nature in all her departments, in relation to this subject; and it was he who first made out the true character of certain pathological phenomena, such as chronic hydrocephalus, and some other states, which had seemed to militate against the doctrine of the mind's organic dependence upon the encephalon; and thus it was he who determined the association of brain and consciousness to be a fact universally true—a principle.[1]

[1] Although almost all rational physiologists believed the brain to be the organ of the mind, yet there were on record numerous facts which, viewed superficially, appeared to militate against this view, and which rendered the *demonstration* of its truth difficult. These cases were first successfully disposed of by means of phrenology. Again, those physiologists who did admit the brain to be connected with the mind, generally regarded it as a single organ, all the parts of which were involved in every mental act. Dr Andrew Combe demolished this idea, in an elaborate paper published in the Transactions of the Phrenological Society, p. 183 (read 9th January 1823). The then prevailing state of opinion on the subject, may be illustrated by a few citations. Mr Rennel (Christian advocate), in his work on Scepticism, page 100, asserted that "it would be difficult to find any one portion of the brain, that has not, in some case or another, been deranged in its structure without any injury to the mind." The Edinburgh Review, No. 48, p. 439, in noticing a paper by Sir E. Home, on the functions of the brain, remarks, that "certain crude ideas are attached to the words Intellectual Faculties, *a vague conjecture arises as to the seat and nature of these faculties,*" &c. Lord Jeffrey may be fairly adduced as rather a favourable specimen of the general intelligence and information prevalent among educated men during the period in which he presided over the Edinburgh Review. Nevertheless, in his article on Phrenology, in the 88th Number of that Journal, he uses these words:—"The truth, we do not scruple to say it, is, *that there is not the smallest reason for supposing that the mind ever operates through the agency of any material organs,* except in its perception of material objects, or in the spontaneous movements of the body which it inhabits; and that this whole science rests upon a postulate or assumption, for which there is neither any shadow of evidence, nor any show of reasoning," p. 257. Lord Brougham,

The next leading principle of Phrenology is, that particular parts of the cerebral mass are the organs of the particular faculties of the mind. Let us now propose the question, whether, if the facts be true, they establish the law, that particular parts of the brain manifest particular faculties of the mind? This principle was arrived at through the fact, that size of organ, *cæteris paribus,* is a measure of functional power. If we grant, or allow it to be proved, that particular muscles execute particular movements, and that size of muscle, other conditions being equal, is a measure of its power, it is impossible to avoid the phrenological conclusion, if the facts be admitted. An application shall here be made to the muscular system, of the language employed by phrenologists in reference to the brain; and it will be seen how corresponding facts are allowed, universally, to authorise analogous inferences in regard to other structures.

"The vigour with which any *muscular action* is manifested is always, other conditions being equal, in a ratio with the size of some corresponding part of the *muscular system;* orders of species *whose movements possess the simplest character*, furnish instances of the simplest constitution of the *muscular system;* whilst, as we ascend in the scale, there is a constant addition of *muscles* corresponding to the multiplicity of the *movements.*"

The corresponding proposition in phrenology is, that the vigour with which any mental power is manifested, is always, other conditions being equal, in a ratio with the

in his "Discourse of Natural Theology," says, "We are authorised to conclude from these facts, that unless some unusual and violent accident interferes, such as a serious illness, or a fatal contusion, the ordinary course of life presents *the mind and the body running courses widely different, and in great part of the time in opposite directions,*" p. 120. He adds, that "this affords strong proof, that the mind is independent of the body." Would educated men write in similar terms now? And if they would not, we have to thank Gall and his disciples for having taught the world a sounder view of the relations between the brain and the mind.

size of a corresponding portion of the brain. The evidence is strengthened by observing that the lower orders of species furnish instances of the simplest constitution of the brain; while, as we ascend in the scale, there is a constant addition of parts corresponding to a multiplicity of faculties, &c., as detailed in former chapters of this work.

If, in the foregoing paragraph, we have that which is adequate to prove certain general principles relative to the muscular system, it is certain that, if the phrenological observations be uncontroverted, they must be allowed to develope the principle, that *the brain is a congeries of organs, some distinct faculty of the mind being manifested through each;* and also the principle, that *size of a cerebral organ,* cæteris paribus, *constitutes a measure of functional power;* and, consequently, it must be granted that phrenology has a higher import and value than would be implied in its mere aptitude to amuse the curious and inquisitive; for, in conformity with every analogy supplied by other departments of inquiry, it must be regarded as involving "knowledge which leads to the development of principles," and entitled, therefore, on the same grounds as chemistry and geology, to rank amongst the inductive sciences.

It has been said that for a principle to be determined, the facts must be universally true; hereupon it may be asked, Do the phrenologists then admit of no exceptions? Do they hold their doctrines to be true in every instance? When they announce the existence of general laws in reference to the functions of the encephalon, of course they maintain them to be established, for all *laws* cease to be regarded as laws upon the occurrence of a sure exception. When, however, mention is made of phrenological *doctrine,* it must be kept in mind that by the term most commonly is understood, not only certain broad inductions, but also the pursuit of these to their remote consequences, by *deduction* from the principles; and it is not contended that even the best writers upon phrenology have always em-

ployed this—the deductive—process aright; any more than it is contended that writers in other departments of science have always been accurate in their deductions.

Before proceeding, however, to any illustration of the distinction to be maintained between deduction and induction, in so far as phrenology is concerned, the author would refer to an inconsistency on the part of antagonists diametrically opposed to the one just discussed. If there be individuals who concede the reality of the leading facts, and yet deny their aptitude for the development of principles, there are others who will apparently accept each of the three generalisations discussed above, and contend at the same time that the facts of the phrenologists are no facts at all. It is the old story of believing in the general principles of phrenology, but not in the details; as if principles had any existence but as manifested in details,—as expressions, indeed, of the conditions under which their universality is to be traced. It is quite time for this *realism* of the mediæval schools to be abandoned. It is certainly competent for any one to contest the accuracy of particular observations, and to retain confidence at the same time in a general principle; but it is anomalous and self-contradictory to maintain a principle abstractedly, and to reject absolutely the only facts which authorise the generalisation from which it springs.

Inductions gained by observation of natural phenomena, constantly furnish a source both of practical and speculative deductions. In tracing principles to their consequences, proximate or remote, a mental process goes on, differing in sóme respects from that which is employed in their primitive discovery. In the one instance, facts are collected, compared, and classified, and the truth which they all exemplify is noted, and expressed in some appropriate formula; in the other, however, the relations subsisting between some general truth and other facts which it does not *prima facie* include, have to be estimated, and the con-

clusion is often attained by collateral aid of several branches of knowledge. Induction ascertains a truth; deduction not only turns it to immediate account, but also leads very often to a partial acquaintance with other circumstances and conditions, respecting which direct experience may have revealed nothing. Thus, the legitimate inductions of astronomers and geologists lead to deductions concerning cosmogony; the truth or falsehood of which, in many instances, leaves the former entirely unaffected. The induction of Dr Marshall Hall, respecting the independent physiological agency of the spinal cord, has yielded materials for extensive deductions in pathology, natural history, and comparative anatomy, some of which may not be true, and yet Dr Hall's principle remain unscathed. Thus it is with Phrenology. For, as this is the science which not only forms the physiology of the brain, but must constitute, moreover, the only sure basis of mental philosophy, it necessarily stands in some relation to every subject which involves considerations either of the mind or the brain; and, under such circumstances, questions of education and corrective discipline, of mental derangement and of jurisprudence, receive considerable elucidation from the light which it affords. In consequence, many important deductions have been made from phrenology by writers upon Insanity, Moral and Mental Philosophy, Criminal Legislation, and other allied subjects; the accuracy or the inaccuracy of which, in particular instances, furnishes no test whatever of the soundness of phrenology as an inductive science.

This discussion may be rendered more intelligible by a little illustration. In the matter of insanity, it is a phrenological doctrine that there is but *partial* disease of the brain in numerous instances, and that treatment should often be based upon this view; and this is a practical deduction so obvious, and so consequent upon the premises, that it is difficult to conceive the existence of any opposition to it, where the inductions are admitted. At the same

time, it is just possible that the reasoning powers in some individual should be weak enough not to recognise so plain a truth, even where the principles of phrenology are allowed; and this too without the individual's involving himself in palpable self-contradiction, for the inability to see the force of the conclusion in question may, in great part, arise from want of that collateral knowlege which is requisite to the adequate perception of it. Then we may suppose the case of some phrenologist arguing that all cases of mental derangement consist essentially in that alteration of structure, which the term inflammation most commonly implies,—a deduction so faulty and premature, that every one properly acquainted with the subject would at once pronounce it to be unsound. And yet its author might consistently maintain his phrenological convictions, notwithstanding the inability to comprehend the facts and principles in their proper relations and consequences.

There are deductions, moreover, from phrenology, the truth or the falsehood of which may not be so obvious as those which have been illustrated in the preceding remarks; and this is the case more particularly, in reasoning upon topics which involve considerations, taken from a great variety of other subjects. Thus, if we take the instance of criminal legislation, some persons will argue from phrenology, that pains and penalties should be altogether abolished, as such, in the treatment of criminals; and they would render prisons, in the fullest and most exclusive sense of the word, houses of *correction*, or moral hospitals, treating the inmates rather as patients, than as offenders to be punished. Others, and Gall amongst the number, have contended for the institution of degrees of severity in the infliction of the capital penalty, in order the more appropriately to influence the sense of fear, in deterring from the commission of crime. Now, whichsoever of these two extreme deductions be nearest the truth, phrenology, as an inductive science, is absolutely unaffected and unconcerned.

The author will take the present opportunity of saying a few words upon the subject of materialism, to which, in the unreasonable apprehensions of many well meaning persons, phrenology has a tendency. It is certainly true, that some able and accomplished phrenologists have made deductions directly involving conclusions of such a character; supporting their arguments, moreover, by considerations taken from phrenology; and yet it is equally sure, that a more numerous class of phrenologists have reasoned in a contrary manner, and maintained that the science leaves the question unaltered relating to the materiality or immateriality of the soul, themselves evincing convictions favouring what is understood by the term spiritualism; and amongst such phrenologists, the great discoverer Gall must be ranked. The present writer may adduce himself as a rigorous and unqualified believer in the mind's distinctness, in essence, from the substance of the brain, and he himself deduces arguments from phrenology, that favour his own views, — of a character more influential with himself, than any others supplied by mere scientific sources; he conceives that plurality of the material organs in coincidence with unity of consciousness, — singleness in the *moi—corroborates* his own convictions. But still he would contend that, philosophically, the mind's materiality or spirituality does not come within the domain of scientific induction at all; and that each individual will ever be likely to form his ideas upon such subjects, either from what is thought to be the teachings of revelation, or from the suggestions of the intuitive consciousness; for, in the present state of existence, we have no endowments that fit us for observing and determining the essence either of spirit or of matter. Indeed, amongst physiologists, the objections to phrenology, on the score of its supposed tendencies to materialism, have for some time been abandoned. It is, now, only amongst parties regarding the mind exclusively in its moral and metaphysical rela-

tions, that such difficulties are started; for it is obvious, that if the body, in its union with the soul, be considered for one moment, identical reasons will be ascertained to exist for coming to conclusions, whatever be the theory adopted in regard to the organic connexion of the two. If the whole corporeal structure be regarded as the simple organ of the soul,—an idea that was prevalent amongst certain schoolmen of the middle ages—the reciprocal influence is decided enough; if the aggregate brain manifest, alike, every mental faculty, the mutual dependence is not the less marked; and if phrenology be accepted as the best and the only scientific exposition of the mind's dependence, in this life, upon organisation, no new principle is asserted in this point of view; the principle maintained upon the other hypotheses, does but sustain an extension, when it is affirmed, that certain conditions of cerebral parts favour the manifestation of particular powers and dispositions inherent in the conscious principle.

The author trusts that he has rendered himself understood, in this attempt to illustrate the distinction to be drawn between the demonstrated or demonstrable axioms raised by the method of *induction*, and the pursuit of these to their real or supposed consequences by *deduction;* because, in the case of phrenology, the whole matter has, again and again, been misunderstood, controverted unfairly, neglected, and reviled, on account of this distinction not being kept in view.

Opponents, indeed, will very rarely dispute the principles of phrenology, excepting in a covert manner; they will rarely even controvert the facts directly; generally speaking, the opposition is exhibited rather in misrepresentation of the whole subject, or in confounding the facts and principles with the defective or vicious reasoning into which phrenologists, like any other class, are liable to fall. In this way, they sometimes find it an easy matter to confute phrenology; " they make their giants first, and

then they slay them ;" but this proceeding no one will hold to be scientific.

In the great majority of instances where a truth is rejected, the result comes partly from some misapprehension of the genuine character of the truth ; and, in reference to the present subject, the causes of such misapprehension rest very much with a class of persons who, without any just pretensions whatsoever, call themselves phrenologists, and proceed to expound the science and the philosophy of cerebral physiology. Some writers, many third-rate lecturers, and numerous well-meaning but ignorant persons, have notoriously taken up phrenology, not from any experimental acquaintance with its real nature or its evidences, nor yet from any accurate knowledge gained at second hand, but because in some instances it has seemed to corroborate anterior prepossessions, and in others, to furnish material for the pettiest inquisitiveness. In the case of science that is recognised universally, such things would hardly occasion a moment's notice or concern ; but when a new branch of science is only advancing towards general recognition, the matter is something different, for the truth itself becomes prejudicial by such circumstances—adversaries, from ignorance or for less creditable reasons, directing their attacks very often against phrenology as it is represented by zealots and half-informed enthusiasts.

Now, when it is considered that phrenology, besides constituting the physiology of the brain, supplies physiognomical indications that furnish the basis of confessedly the most useful philosophy of the mind that has yet been propounded, it should excite no surprise that its exposition and pursuit have fallen so much into the hands of persons totally incompetent to deal with it in some of its higher relations. Members of the medical profession, however, are the very last persons who should take advantage of this circumstance to neglect or to misrepresent the subject as it exists in nature. As a practical pursuit.

phrenology rightly belongs to them; and if the *form* in which it is very often presented to the public be both unscientific and unphilosophical, it is they who are responsible, and not Gall's physiology, when they neglect to acquaint themselves with its true merits. It is readily conceded that, in many of the popular representations of this subject, there is very often, in the affair of fact, much error and overstatement; and in the reasoning, grievous confusion of induction and hypothesis; but this circumstance does not authorise disregard of its just pretensions, on the part of a numerous class of persons who ought to inform themselves of its true character.

It scarcely falls within the scope of the present writer's design to touch upon certain other points which, in connexion with phrenology, are sometimes adduced as characterising its dangerous tendencies; reference is made to such subjects as fatalism, philosophical necessity, moral liberty, revealed religion, or pure theism,—questions, which unhappily are believed by many well-intentioned individuals to be affected prejudicially, in relation to the common good, by the doctrine of phrenology. In regard, however, to all such collateral matters, the remarks previously submitted in regard to materialism will substantially apply here. Gall's physiology is erected upon numerous and readily observed facts, arranged, compared, and classified, so as to evolve certain general principles, or truths of universal application; the facts themselves are just competent to develope the principles of phrenology; these they prove; but, inductively, they can do no more. On the various points just glanced at, it will generally be found that the convictions of individuals have been deduced, primarily, from other than phrenological considerations, whatever they themselves may occasionally think of the matter. Whatever views people may entertain on such subjects, they will be likely to illustrate and partially defend them by appeals to phrenology, if they have a

knowledge of this science; but the present writer is much mistaken if the speculative conviction has often been deduced from it, in the first instance; for, in point of fact, all such questions as fatalism, moral liberty, deism, and revelation, have no direct or necessary connexion with physiological discussions; they remain just where they were before the brain's physiology was discovered; so far as demonstration and induction can be brought to bear upon them, their aspect is entirely unchanged by phrenology.

The author may probably convey his meaning yet more clearly, if he illustrate it by example. Mr Combe's Constitution of Man is undoubtedly the most diffused work in existence, setting forth profound philosophical views, deduced chiefly from phrenology. Now, it is certain that many individuals regard every thing contained in that book as phrenology itself, and determine their acceptance or rejection of Gall's physiology of the brain, by the character of its contents; notwithstanding that Mr Combe, in anticipation of such a possible result, warns the reader against all such confounding of positive science with an individual's reasoning. To some of Mr Combe's views, as propounded in the work in question, it is well known that many persons object; and some from this cause are absurd enough to reject phrenology, as if it were something proper to Mr Combe rather than a great fact in nature. Be that gentleman's philosophical reasonings just and true, or be they not, the evidences of phrenology, as an inductive science, are utterly unaffected. Any one who *scientifically* acquaints himself with Gall's discovery sees this at once. The late Mr William Scott of Edinburgh, one of those in that city who early recognised and admitted the truth of phrenology, and who never swerved from his convictions upon the subject, wrote a book to controvert some of Mr Combe's positions; employing phrenology, moreover, in the line of argument which he adopted. In

the Constitution of Man, and in many other phrenological writings, there are philosophical views advanced to which, however ably maintained, the present writer, as he understands them, can by no means assent; yet will any one doubt, after perusal of this book, that he is a sound and orthodox phrenologist? In quitting this topic, he would remark that, whilst concerning the facts of phrenology and its principles as an inductive science, there may ultimately be concord and unanimity amongst physiologists, the deductions from these, on the part of individuals, must, and will, ever vary, according to their other views upon the several subjects in the discussion of which they apply its aids. It is no part of the author's plan to discuss the validity or the fallacy of the deductions which individual phrenologists have made in reasoning from their science.

The surest test, probably, of the soundness of particular deductions, is the success with which they can be applied in practice; and, regarded in this point of view, it is quite certain that many most valuable ideas have been deduced from phrenology. The present writer is well assured both from reason and experience, that, in the prevention, discrimination, and treatment of insanity, and other nervous affections, in education, in the prevention of crime, and in prison discipline, the present science is susceptible of much valuable application. The practical utility of phrenology, in some of these respects, has already been illustrated in earlier portions of this work. The great aid which this source supplies to the medical practitioner, in the management of a numerous and important class of diseases, the author can amply attest from his own experience; but, for several reasons, he forbears to dilate upon this theme. And, indeed, his present object is rather to indicate the general applications of phrenology, than to go into actual detail; for to do justice to this latter subject would require volumes rather than a small section of a single one; and,

besides, he does not feel that, in this respect, he could advantageously add to the literature already in existence.

It is a very interesting circumstance to notice, that *all* who have to deal practically and professionally with mind, and who at the same time understand phrenology, recognise the light which it sheds on their path. In illustration of this fact, some valuable communications will be found in an appendix to Dr A. Combe's Address to the Students of Anderson's University,[1] consisting of letters from distinguished individuals addressed to Mr George Combe, testifying not only to the truth, but to the *practical usefulness* of phrenology. Mr Richard Carmichael of Dublin, President of the Royal College of Surgeons in Ireland, and eminently a *practical* man, writes as follows:—" Indeed, the assistance it lends in establishing a confidence in ourselves, and acquiring the confidence of our patients, is of the greatest utility in the treatment of those ailments which depend upon a morbid state of the brain, or some other portion of the nervous system, such as epilepsy, hysteria, hypochondriacism, and neuralgia." Dr W. A. F. Browne, who has for many years been engaged in the practical management of the insane, being physician to the Crichton Institution, Dumfries, says,—" In the exercise of my profession, I have been enabled, by the aid of phrenology, to be of essential service in directing the education of the young, as a protection against nervous disease, and in removing or alleviating the various forms assumed by insanity in the mature. For many years I have devoted myself to the study of mental diseases, and to the care of the insane. During my studies at Salpetrière, Charenton, &c., in Paris, I was able to derive great additional information from my previous knowledge

[1] Phrenology—Its Nature and Uses. An Address to the students of Anderson's University, at the opening of Dr Weir's First Course of Lectures on Phrenology in that Institution. Jan. 7, 1846. By Andrew Combe, M.D. Edinburgh, 1846.

of phrenology; and since I have been entrusted with the care of public asylums, I am inclined to attribute whatever success may have attended my efforts to ameliorate the condition of those confided to my charge, to the same cause." Dr W. B. Hodgson, the able and intelligent Principal of the Liverpool Mechanics' Institution, writes: "Of the utility of phrenology in various pursuits, there are not now wanting many influential witnesses. Of its importance to the educationist, I may speak, if with humility, yet with confidence based on actual experience. To the practical teacher phrenology is of eminent service, not merely in enabling him to form rapid and correct judgments of individual characters, but from its clear and simple philosophy of mind, the light it throws on the *nature of the being to be instructed*, and consequently on the true aim and wisest methods of education." Dr Conolly, the well-known physician to the Hanwell Lunatic Asylum, whose calm judgment and philosophic spirit render him, in many respects, the very model of a practical physician, addresses Mr Combe, in attestation of the advantages to be derived from phrenology, in the following terms:—"Many and pressing avocations leave me no time just at present to express to you, in a manner at all worthy, my conviction of the great usefulness of habitual regard to the principles of phrenology, especially in my department of practice, and of the confusion and imperfection of the views which seem to me to be taken both of sound and unsound mind, by those who reject the aid of observations confirmed now by vast experience, and most of which may be daily verified in asylums for the insane. I am also convinced, that attention to the form of the head, conjoined with that cautious consideration of all other physical circumstances which no prudent phrenologist disregards, will often enable the practitioner to form an accurate prognosis in cases of mental disorder, and to foretell the chances of recovery or amelioration, or hopeless and gradual deterio-

ration. But I am aware that I am now taking a very limited view of the applications of the science; which, however, I know you will excuse, in consideration of the somewhat exclusive occupation of my mind on these subjects." Dr William Gregory, Professor of Chemistry in the University of Edinburgh, expresses himself thus:—"Phrenology furnishes the key, so long sought for in vain, to many perplexing facts; explains, in a natural and simple manner, the phenomena of partial genius, and of partial insanity; throws equal light on innate tendencies, whether intellectual, moral, or sensual; yields the most precious hints for the treatment of the insane, as well as of the criminal; and, finally, forms the only rational foundation for an enlightened education."

Now, does any one suppose that Dr Carus, or Dr Carpenter, or Dr Todd and Mr Bowman, or any one else having, like these physiologists, rejected the physiology of Gall, could procure such testimony in favour of *their* physiology of the brain, as that which the above extracts furnish? Nay, would they themselves even write in such terms of their own doctrines? That a *false* physiology should be so *useful*, and a *true* one so *useless*, contradicts all rules of philosophy and all experience.

Although, in the preceding chapter, there are some indications advanced of the extent to which phrenology will apply in the estimate of individual character, some additional remarks, it appears to the author, may not here be out of place. And it is to be observed that, under all circumstances, it is surprising in how near an approach to accuracy, a particular examination of the head will often issue; so often, indeed, that the results are eminently calculated to satisfy any thoughtful mind, that at least there is *something in phrenology*, and, moreover, to enforce the obligation upon the student of inquiring, *how much?* It is impossible fairly to suppose that the coincidence of cranial configuration and predicated character, in so many instances,

is nothing but accident. The author is too little of a mathematician to say how often, on the chance hypothesis, correct conclusions should be gained. Not, certainly, in half the experiments; nor yet in a fourth; nor would it, probably, be once in ten times. Yet a good practical phrenologist, not attempting too much, will decide upon natural character in a very large majority of trials. To a great extent, this has been admitted by opponents themselves. Dr Roget, a very old and persevering enemy of Gall's discovery, furnishes an example of this circumstance. In a letter addressed to Mr Combe in the year 1819, and republished among other documents appended to the translation of Gall on the Cerebellum, Dr Roget says: "I have no confidence in the authors of the system, either as faithful observers, or as sound reasoners; and, so far as I have myself had an opportunity of comparing the results they pretend to have obtained, I cannot say that I have found them in any degree verified." And yet, notwithstanding this *professed* inability to corroborate, "in any degree," the phrenological observations, he goes on to say, in the very next sentence: "I at one time took some pains to make observations on this subject; and am persuaded that I met, in the course of them, *nearly* as many exceptions to the rules, as instances in confirmation of them." Dr Carpenter, upon the same subject, says: "The author's own experience of their determinations, however, has certainly led him to the belief that failure is *nearly* as frequent as success."[1]. The term "nearly," in the cautious language of Drs Roget and Carpenter, plainly implies that, in a majority of instances witnessed by themselves, the phrenological statements were corroborated. Indeed, when all the difficulties are remembered that stand in the way of a declaration of character that shall *strike*, in a large proportion of cases, in which nothing remarkable may be

[1] Human Physiology, p. 238.

indicated by the head, the wonder is, not that strict accuracy should not *always* mark the result, but that this should be the fact so often as it is.

Whilst upon the present subject, the author would record his protest against the very unfair practice of opponents, who persist in judging of phrenology and its evidences, by the results of examining heads on the part of a class of persons calling themselves phrenologists, but who have about as just a title to the designation, as the swarm of empirics of another class have to the title of Doctor, that infest society, and pollute the walls of our cities, and the columns of the newspaper press, with their mendacious and obscene advertisements. It is true that phrenological quacks are run after by all classes of persons, and that phrenology is very often most absurdly adopted or rejected, according to the supposed degree of accuracy, characterising particular charts of development obtained from such persons. But members of the medical profession do most unjustly and inconsistently quarrel with Gall's physiology of the brain, on such grounds; let them conceive the reply they are prepared to make, in case of the argument being applied by analogy, *ad hominem*. Are we of the medical profession ashamed of our science, and do we therefore neglect it, because ignorance and presumption within our ranks sometimes, and folly and imposture external to us very often, "play fantastic tricks?" Assuredly not; most egregiously foolish would be the adoption of any such course; and, *mutatis mutandis*, phrenology should be regarded in a like point of view, in reference to the circumstances alluded to.

The scientific phrenologist, indeed, would never appeal to examinations of the ordinary run of heads for *proofs* of phrenology; if a detailed examination of the head be indiscriminately made, it is always *as an experiment*, and not as a means of gaining positive evidence. As in demonstrating to the philosophical sceptic the efficacy of the

Materia Medica, it is always an unsatisfactory procedure to reason from results of combinations of different substances, unless there be a very decided predominance of some one article; yet the action of individual medicines once satisfactorily recognised upon appropriate grounds, we may experiment upon and trace the agency of the same things in any degree or kind of combination; so, in like manner, although general examinations of the head cannot uniformly be expected to furnish sure and undoubted results, it is yet certain that where strongly marked peculiarity of individual character exists, the external indications, in appropriate subjects, may always be detected; but, nevertheless, such instances, from the very nature of the case, should not be anticipated as the invariable rule. It has been seen in what way Gall *discovered;* the inquirer who seeks for philosophical *proof*, will repeat the discoverer's procedure, and will study combinations for instruction, according to what he may find, and not with the view of *testing* phrenology.

Some striking illustrations of the kind of evidence furnished by cases of extreme development, and of the class of facts which an honest sceptic should investigate, when the object is to attain a determinate conclusion respecting the truth, or the falsehood, of Gall's doctrine, were supplied by Mr Combe, in the year 1829. During a visit to Dublin, in the spring of that year, he examined the heads of numerous inmates of the Richmond Lunatic Asylum, and of the Penitentiary, in that city; publishing a narrative of the proceedings in the Phrenological Journal of the following August.[1] The account in question has been long before the world, and statements more or less parallel have many times been made: yet such is the remarkable character of Mr Combe's procedure; so well does it illustrate the true philosophical method adapted for inquisitions of the kind to which it relates; and so admirably does it

[1] Vol. vi. p. 80.

corroborate the justice of much advanced in the present treatise, that the author conceives that an obligation will be done to the reader by subjoining the narrative referred to; more especially, as he has reason to know that physiologists eminent for their talents and attainments, who yet venture to discuss phrenology in an unfavourable tone, are entirely unacquainted with the existence of such facts as the account embodies. Mr Combe's own remarks, prefatory to the actual narrative, supersede the necessity of further observations upon the matter from the present writer.

" *Richmond Lunatic Asylum, Dublin.*

This Asylum was founded about 14 years ago, for the reception of pauper lunatics. It contains at present above 300 patients. None are admitted without a medical certificate of insanity, and an affidavit of poverty. Dr Jackson is the physician, and Mr Johnston the surgeon. Dr Crawford acts at present as Dr Jackson's temporary substitute.

Mr Combe visited this institution on 20th April, 1829, in presence of Dr Crawford,—Mr Grace, the Moral Governor,—Major Edgeworth, Governor of the House of Industry,—Dr Cumming, Assistant Physician to ditto, and Dr Mollan.

Dr Crawford stated, that he had written down the characteristic features of several cases of insanity, and proposed that Mr Combe should examine the heads of the patients and deliver his opinion on them, and thereafter, that the written notes and Mr Combe's remarks should be compared.

Mr Combe observed, that he had no objection to make the experiment for the sake of gaining information; that a small organ may become diseased as well as a large one, and in these instances the head would not indicate the features of the alienation; however, that disproportionate de-

velopment of particular organs is itself one cause of insanity; and, besides, when any organs are particularly large, the feelings connected with them generally show themselves strongly in the manifestations, even although they should not be the seat of diseased affections; that, therefore, in the majority of cases, the form of the brain ought to present indications of the mental condition of the patients, and that with these explantions he was ready to proceed. Mr C. added, that although he had on other occasions examined the heads of a number of insane patients, he had not before attempted to predicate the features of the insanity from the heads, and therefore this was to himself an experiment; that he did not know how far he might succeed, but it was interesting to ascertain what effect the mere form of the brain produced in insanity; that his inexperience threw obstacles in the way of his success, but that in science experience was to be obtained only by practice. He said that he would point out and write down the organs that were prominently developed or very deficient in each case, and then make his remarks; after which he would be glad to see the notes prepared before his arrival.

The hair of the patients was in general worn short, and the facilities of observation were thus increased.

We print the observations of Mr Combe and the notes of Dr Crawford in separate columns, in the order in which the patients were introduced. Mr Combe underlined the organs on which he considered the insanity to depend, and these are printed in italics.

In every instance Mr Combe pointed out the development to all the gentlemen present. He made them feel it, and contrast it with the heads of other individuals in the room; and requested them to judge for themselves, and not to look on him as performing a stage-trick, or doing any thing which they could not learn to do as well as he, or far better, as their opportunities were superior.

Mr Combe's Remarks.	*Dr Crawford's Remarks.*
Patient's name, Lynch. Largest organs, *Self-esteem* *Wonder* *Causality* *Language* *Combativeness* Also large: Amativeness Philoprogenitiveness Concentrativeness Acquisitiveness Love of Approbation Firmness Full: Veneration Deficient: Conscientiousness Mr Combe said, that he considered *Wonder,* which, when diseased, gives notions of supernatural agency and inspiration, and *Self-esteem,* as probably the leading sources of alienation in this case; that Causality and Language should also be conspicuously manifested.	Patrick Lynch, aged 42, a cooper. Two and a half years ill. Married, and has children. Monomania. *Religious pride,* with vivid imagination and the highest degree of excitement, requiring restraint; fancies himself inspired and endowed with omnipotence; frequent hallucinations; visits from heaven, &c.; great flow of language in a style quite superior to his rank in life; drinking the cause of his illness; second attack. Note.—" Dr Gall remarked in the first fanatic who fell under his observation, a large development of the part of the brain now marked 'Wonder,' and subsequently met with many similar instances."—See Combe's System, p. 226.—Editor.
Patient's name, E. S.[1] Large: Amativeness Philoprogenitiveness Very large: *Destructiveness* *Combativeness* Large: Self-esteem Cautiousness Moral organs deficient, particularly: Veneration and Hope Moderate: Conscientiousness Of the moral organs: Benevolence is rather well developed Intellectual organs: Rather well developed The patient was withdrawn, and Mr Combe added: This is the worst head I ever saw. The combination is worse than Hare's.—Combativeness and Destructiveness are fearfully large, and the moral organs altogether very deficient: Benevolence is the best developed of them, but it is miserably small compared with the organs of Combativeness and Destructiveness. I am surprised that that man was not executed before he became insane.	Patient E. S., aged 34. Ten years since first admission. Total want of moral feeling and principle, great depravity of character, leading to the indulgence of every vice, and to the commission even of *crime.* Considerable intelligence, ingenuity, and plausibility; a scourge to his family from childhood; turned out of the army as an incorrigible villain; attempted the life of a soldier; repeatedly flogged; has since attempted to poison his father. (See letter from Dr Crawford about this patient, Article XV. of this Number.—Editor.)

1 Where the names are not published, the patients have relations, out of delicacy to whom they are withheld.

Mr Combe's Remarks.

Patient's name, Dowling.

Enormously large,	*Self-esteem*
Ditto	*Firmness*
Large	Amativeness
	Combativeness
Rather large	Destructiveness
	Adhesiveness
	Benevolenee
Full	Acquisitiveness
Pretty good	Intellect
Deficient	Cautiousness

The organs in great excess are Self-esteem and Firmness.

Dr Crawford's Remarks.

Joseph Dowling, silk-weaver, aged 29. Two years ill. Unmarried.

Monomania. High pride. An emperor. Very overbearing, quarrelsome, and dangerous, but is easily tamed.

Note.—"When the organ of Self-esteem becomes excited by disease, the individual imagines himself to be a king, an emperor, a transeeendent genius, or even the Supreme Being."—Combe's System, p. 164.—Editor.

Patient's name, Bayley.

Enormously large,	*Destructiveness*
Ditto	Secretiveness
Ditto	Acquisitiveness
Large	Self-esteem
Ditto	Amativeness
Deficient	Moral organs, particularly
Very deficient	Benevolence, but
Larger	Veneration
Moderate	Reflecting organs
Pretty large	Knowing organs

The combination here is very bad, the animal organs greatly preponderate; Destructiveness is the largest.

George Bayley, clerk, aged 39; nine years since last attack. Relapsed twice.

Mania. Intermittent. Very violent, pugnacious, and destructive; sullen and morose. An incorrigible drunkard and immoral charaeter.

Patient's name, Edmundson.

Very large	Destructiveness
Ditto	Love of Approbation
I have a difficulty as to these organs, as they are irregular in development	Hope very large Wonder, back part of it considerable Ideality, back part of it large
Large	Amativeness
	Adhesiveness
	Combativeness
	Cautiousness
Moderate	Veneration
Deficient	Bencvolence
	Conscientiousness
	Refleeting intellcct
Moderate	Knowiug intellect

It is difficult to point out the leading features of this case. The orgaus of Hope, Wonder, and Ideality, present an unusual appearance, corresponding to malconformation.

Christopher Edmundson, clerk to a merchant, aged 47. Twelve years ill. Unmarried.

Monomania, religious. Fancied himself Jesus Christ, and attempted to walk on the sea and fast for forty days. Imagines now that his body is inhabited by the spirit of another person; was a clerk and methodist, and gave up his employment to go about preaching and working miracles. At times irritable and violent.

Patient's name, Brady.

Deficient	Combativeness
Ditto	Hope
Ditto	Veneration

George Brady, servant, aged 37. Twelve years since first attack. Has relapsed. Unmarried.

Melancholy. Great timidity of dis-

Mr Combe's Remarks.

Very deficient	Ideality
Ditto	Tune
Ditto	Wit
Large	Self-esteem
Ditto	Firmness
Rather large	Benevolence
Ditto	Conscientiousness
Considerable	Cautiousness
Large	Individuality
Ditto	Eventuality

The deficient Combativeness, Hope, Veneration, and Ideality, and Wit, large Cautiousness and Conscientiousness, will predispose to melancholy.

Dr Crawford's Remarks.

position. Fancies he was accused of theft, and has constant apprehension of punishment, either human or divine. A variety of hallucinations on this subject. Gentle and kind. His master, to whom he was butler, was robbed, and although the thief was discovered, this occasioned his mental derangement.

Patient's name, Mulligan.

Large	Philoprogenitiveness
Ditto	*Secretiveness*
Ditto	*Cautiousness*
Rather large	Acquisitiveness
Deficient	*Hope*

This is another case of melancholy.

Matthew Mulligan, aged 39, cabinet-maker. One and a half years ill.

Melancholy. Religious despondency. Very silent; uncommunicative; suspicious and morose. Is married, and has a family; for the fate of whom he shows great anxiety.

Patient's name, Petit.

Very large	*Self-esteem*
Large	*Combativeness*
	Love of Approbation
	Hope
	Ideality
Rather large	*Wit*
Large	Philoprogenitiveness
	Adhesiveness
	Firmness
	Locality
	Comparison
Full	Conscientiousness
Deficient	Secretiveness

I feel a difficulty in pointing out particular organs in this case, but have marked (in italics) those that seem most likely to determine the character.

James Petit, shopkeeper, aged 32. One year ill. Married. Has one child.

Mania, intermittent. Very violent, combative, and brutal. A high opinion of himself. Fancies he has great riches, and was giving away all his property to the ruin of his family. Has a good deal of humour, by the excitement of which his violence is easily subdued.

Patient's name, Fogharty.

Large	*Destructiveness*
Ditto	Love of Approbation
Ditto	Cautiousness
Ditto	*Wonder*, particularly on one side.
Ditto	Imitation
Deficient	Hope
Ditto	Veneration
Well developed	Intellect

The leading organs in this case are *Wonder* and *Destructiveness*.

Thomas Fogharty, a marine and tailor, aged 39. Ten years ill.

Monomania, with the singular delusion of his being the *Almighty*. Says he had no beginning and is never to die; that he can bestow immortality on whom he chooses; is very irascible, and threatens those who offend him with hell-fire and brimstone. An extraordinary combination of servility and humility with these exalted notions. Very mean in his appearance; works as a tailor, and is an importunate beggar. Illness brought on by excessive drinking.

Mr Combe's Remarks.

Patient's name, Duff.

Very large	Self-esteem
Ditto	Firmness
Ditto	Secretiveness
Large	Destructiveness
Ditto	Cautiousness
Deficient	Hope
Small	Ideality
Ditto	Wit
Ditto	Philoprogenitiveness
Moderate	Veneration

The combination here is that which is described in the works of Phrenology, as leading to melancholy and suicide.

Dr Crawford's Remarks.

Bryan Duff, collector of minerals, aged 31. Three years ill.

Melancholy. Deepest dejection. Silent, morose, inactive. Attempted suicide, and to destroy his own child. After disappointment in his business took to drinking; was seized with maniacal delirium, which has sunk into permanent melancholy.

Patient's name, M'Evoy.

Mr Combe pointed out that this head was regularly developed, that he could not fix upon any organs as excessive either in largeness or deficiency. He wrote down the organs which were most developed as follows:—

Large	Destructiveness
	Secretiveness
	Love of Approbation
Rather large	Acquisitiveness
	Cautiousness
Full	Constructiveness
Moderate	Hope
Rather small	Self-esteem
Considerable	Wonder
Large	Knowing Intellect
Full	Reflecting do.

Michael M'Evoy, aged 28. Ill nearly one year.

Melancholy. Very silent; disinclined to exercise, and the air of being absorbed in thought. No one prominent delusion. Is gradually recovering.

Patient's name, R. J. C.

Very large	Combativeness
	Destructiveness
Large	Amativeness
	Self-esteem
	Veneration
	Firmness
	Cautiousness
Full	Ideality
Rather full	Philoprogenitiveness
Deficient	Conscientiousness

Patient's name, R. J. C., aged 34. Four years ill.

Mania, intermittent. During the paroxysms he conceives himself to be Jesus Christ, and is the most furious, treacherous, and dangerous patient in the institution. Is very athletic and muscular, and not easily restrained. The violence of the paroxysms lasts only a few days. During the intervals he is free from mental aberration, quiet and inoffensive; was addicted to drinking.

FEMALES.

April 21, 1829.

Patient's name, Hall.

Very large	*Self-esteem*
Large	Concentrativeness
Ditto	Destructiveness

FEMALES.

Jane Hall, aged 48. Ill 8 years.

Monomania. Pride. Queen of France. Hallucinations about rebels surrounding the house. Fancies she

Mr Combe's Remarks.

Large	Hope
Ditto	Veneration
Full	Wonder
Fairly developed	Intellect

The organ of Self-esteem is here by far the most predominant.

Dr Crawford's Remarks.

has rats inside her forehead. Generally cheerful and quiet. Illness occasioned by fright during disturbances in her country.

Patient's name, C. C.

Enormously large	*Love of Approbation*
Large	Benevolence
Ditto	Imitation
Considerable	Wonder
Moderate	Self-esteem
Ditto	Veneration
Good	Intellect

The Love of Approbation is here out of all ordinary proportion to the other organs.

Patient's name, C. C., aged 30. Ill 3 years.

Monomania. An air of great self-importance. Fancies herself entitled to a fortune withheld from her by her father. Conceived a conspiracy to be formed in the country against her life, and wished to give information on the subject to government. Is very vain of her person. Her mind appears constantly engaged in great plans. Her connexions are very respectable. She was domineering in her family, and quarrelled with her step-mother.

Patient's name, Dunn.

Very large	Destructiveness
Ditto	Secretiveness
Large	Amativeness
Ditto	Philoprogenitiveness
Ditto	Combativeness
Ditto	Self-esteem
Moderate	Love of Approbation
Rather deficient	Veneration
Ditto	Hope

Fairly developed intellectual organs.

Eliza Dunn, aged 56, 9 years ill.

Monomania. Pride. Fancying herself at one time a queen, at another Ali Pacha, the Dey of Algiers, or the Grand Turk. Calls the attendants about her uncle Paul, or by the name of some great lord or lady. Very lively and loquacious, but not violent.

After Mr Crawford's remarks were read, Mr Combe observed, that in this case the development had not afforded an index to the leading features of the insanity; and that, as formerly mentioned, diseased affection of particular organs may exist which is not indicated by external signs.

Patient's name, Nelson.

Large	Philoprogenitiveness
Ditto	Adhesiveness on one side, and small on the other.
Ditto	*Cautiousness*
Ditto	Love of Approbation
Ditto	*Conscientiousness*
Deficient	*Hope*
Ditto	Ideality
Small	Intellect

Mr Combe remarked, that this combination indicated melancholy.

Eliza Nelson, aged 40. Ill ten months.

Melancholy after the death of her husband.

Mr Combe's Remarks.

Patient's name, J. H.

Large	*Adhesiveness*
Ditto	Destructiveness
Ditto	Secretiveness
Ditto	*Cautiousness*
Ditto	*Self-esteem*
Ditto	*Love of Approbation*
Deficient	*Hope*
Fairly developed	Intellect

Mr Combe stated, that he had underlined the organs by which the insanity would be characterised.

Dr Crawford's Remarks.

Patient's name, J. H., aged 41. Ill three years.

Monomania, with pride. Occasional high excitement. Attempted her husband's life with a knife from jealousy, and also threatened to destroy her children. Is of low birth, and without education, and married a dissenting clergyman.

Patient's name, Gallaher.

Mr Combe stated, that this head was irregularly formed, and he had great difficulty in stating what the development of the different organs was. He then wrote as follows:—

Large	Amativeness
	Adhesiveness
	Combativeness
	Destructiveness
	Self-esteem
	Love of Approbation
	Cautiousness
Uncertain on one side, probably large	Wonder
Deficient	Hope on one side
Uncertain	Ideality
Not so large	Secretiveness as Cautiousness
Doubtful	Acquisitiveness, but, I think, large.

Susan Gallaher, aged 23. Six months ill.

Monomania. High religious excitement, with pride. Imagines that the welfare of the people of her country depends upon her; that she has received revelations from Heaven, informing her of a conspiracy against their lives and property. Has seen a bright light in her room, from which a voice proceeded. Very excited and destructive, requiring coercion. Very importunate to be sent home.

Patient's name, Mooney.

Rather large	Amativeness
Ditto	Self-esteem
Large	Love of Approbation
	Secretiveness
	Destructiveness
	Wonder on one side
Deficient	Cautiousness
Moderate	Combativeness
Full	Ideality

Alicia Mooney, aged 30 years. Five years ill.

Monomania, and occasional maniacal paroxysms. Fancies she has plenty of money, *and is exceedingly importunate to be allowed to return home.*

Patient's name, M'Aveeny.

Large	Philoprogenitiveness
	Love of Approbation
Very large	Firmness

Indifferently-formed head, but nothing very remarkable.

Ellen M'Aveeny, aged 28. Four months ill.

Puerperal mania. Cheerful and humorous, but restless and destructive, and very positive, requiring coercion.

Patient's name, Kelly.

Very large	*Love of Approbation*

Ann Kelly, aged 37. Two years ill.

Mr Combe's Remarks.

Large	Secretiveness
	Self-esteem
	Destructiveness
	Ideality
	Imitation
	Constructiveness
Large	Destructiveness
	Firmness
Full	Benevolence
	Veneration

Part of the brain, supposed to be Wonder, *large*, or it may be Hope. Hope moderately developed, unless as before.

The characteristic organs in this head are Self-esteem and Love of Approbation, as one combination, and Constructiveness, Imitation, and Ideality, as another.

Dr Crawford's Remarks.

Monomania. Pride. Imagines she is *Napoleon*. Very irascible, but easily calmed by a little praise. Dresses partly as a man. Speaks of herself as a man, and in the third person. Has made herself trowsers, and a highly-ornamented cloak with simple threads. Will never wear a cap.

Patient's name, Thomas.

Very large	*Amativeness*
Large	Philoprogenitiveness on one side, and full on the other
Very large	*Self-esteem*
	Love of Approbation
Large	Firmness
	Destructiveness
	Veneration
Full	Cautiousness

The leading organs in this case are Amativeness, Self-esteem, and Love of Approbation. Philoprogenitiveness is unequally developed.

Aurelia Thomas, aged 34 years. Five years ill.

Monomania. Great anxiety after her children. Fancied they were starving outside the house, and that she heard their cries, and insisted constantly upon her food being taken to them. Occasionally very violent and outrageous. Disappointment after seduction the supposed cause of her illness. Has ceased now to inquire after her children, saying, that a voice from heaven had informed her that they were dead, and in heaven.

Patient's name, O'Neil.

Largest organ is Destructiveness. None others very remarkable.

Mary A. O'Neil, aged 35. Nine years ill.

Mania; high excitement, and very destructive; requires constant restraint; very abusive, and passionate; no appearance of moral or intellectual feeling; rather fatuous.

Visit to the Penitentiary, Dublin, 29th April, 1829.

Mr Combe visited the Penitentiary, in presence of the Governor, the Chaplain, Dr Crawford, Dr Cumming, Dr Mollan, Mr Grace, Major Edgeworth, Dr Duncan, &c. After looking at the arrangements of the house, which seemed excellent, and seeing the prisoners at work, Mr C. requested that any ten or twelve convicts who were nearest

the apartment in which he then was might be introduced. This was accordingly done. Mr Combe placed the convicts on a form, and requested any of the visitors to sit down on the same seat, with several on each side. Dr —— did so, and Mr Combe pointed out that the organs of the animal propensities lying at the base of the brain were larger, and those of the moral sentiments, lying on the top of the head, relatively smaller in the heads of the convicts generally, than in that of Dr —— and the other visitors present. The gentlemen in the room examined the heads, and recognised this difference as an obvious characteristic.

Mr Combe then examined more minutely the heads of these convicts. He noted down in pencil the particular organs which were remarkable for great size or deficiency relatively to the others, and also the general qualities which he inferred from the combination of the whole organs in each head. This course was followed first with several male, and thereafter with several female convicts. Mr C. pointed out the appearances of the heads to the gentlemen present, but no observations on the characters which they indicated were made in presence of the prisoners. The party retired to the Governor's house. Mr C. then read aloud his remarks, on which the Governor and Chaplain delivered an instantaneous opinion. Mr Combe subsequently transcribed his notes, and sent them through Dr Crawford to the Governor, who very kindly returned them with a written report, and the following letter:—

HILL W. ROWAN, Esq. to GEO. COMBE, Esq.

Richmond General Penitentiary,
Dublin, 28th May 1829.

Dear Sir,—Dr Crawford has favoured me with the perusal of your observations respecting the dispositions and propensities of the individuals whose heads you examined in this institution, with a view to phrenological inquiry.

I have given to that gentleman a short statement of my opinions respecting the same persons, formed from close observation of their dispositions and conduct for several years; and it will probably be satisfactory to those who are impressed with your opinions on the subject of Phrenology, to find that *my experience* corroborates, in almost every instance, what it appears Phrenology would predicate of the individuals in question.

It may be right to add, that I wrote the substance of my observations respecting the great majority of the persons whose heads you examined *before* having read your paper. Indeed, Dr Crawford concurred with me in opinion, that such would be the most satisfactory course to pursue.

The Penitentiary, respecting which you desire to have some information, is a national government establishment, on the principle of the Millbank General Penitentiary in London.—I am, &c.

HILL W. ROWAN, *Governor.*

The following are Mr Combe's remarks, and Mr Rowan's report:—

Mr Combe's Remarks.—Boy, No. 1, G. K.—This individual has large Acquisitiveness and Secretiveness. He probably has been a thief; but the development of the moral organs is considerable: he may be much improved by moral and religious education.

Governor's Report.—No. 1, G. K.—This boy's conduct has been almost invariably correct since his confinement. He was convicted, along with his brother, of the crime with which he was charged; and I have no doubt was led into it by his brother's conduct and importunities. He is lively, thoughtless, and obliging—hasty in his temper, but peaceable—with very good intellectual powers—argumentative, and a little cunning. Convicted of larceny.

Mr Combe's Remarks.—No. 2, J. K.—In this individual there are large organs of Cautiousness, Secretiveness, and Acquisitiveness; good Intellect; a deficient Combativeness, not large Destructiveness, with very small Conscientiousness and deficiency of the moral organs in general. Has the dispositions of a thief, but he will scarcely have the courage to steal in his own person.

Governor's Report.—No. 2, J. K.—This young man's conduct has been generally correct since his confinement; but I apprehend that this is occasioned as much by cautiousness of disposition, good looking to, and fear of correction, as from any good qualities he may possess. I have learned from another prisoner now in confinement, that his influence led his brother (No. 1), into the perpetration of several small felonies. His habit was to commit small thefts, and having deposited the stolen goods with other persons, by cunning and ingenuity to throw on them the imputation of the theft. He is intelligent, and apt to learn. He was once very anxious to be sent to Botany Bay, from a conviction, as he stated, that, when discharged from hence, he would be guilty of fresh crimes. He was convicted of larceny.

Mr Combe's Remarks.—No. 3, P. K.—This boy has large Acquisitiveness and Secretiveness, and large organs of Intellect and Imitation; but he is very deficient in Conscientiousness. He closely resembles the boy, John Gibson (who is mentioned in the phrenological works, and a cast of whose head Mr C. had exhibited at a lecture two days before). He has the talents and dispositions of an expert swindler.[1]

Governor's Report.—No. 3, P. K.—This is in almost all

[1] We repeat, that Mr Combe, in drawing his inferences, considered the *whole development* of the head in each case, and did not found on the particular organs alone which he noted as predominant. He explained this to the gentlemen accompanying him.

respects a very *bad boy;* he is addicted to swearing, lying, gambling, and every kind of meanness and duplicity. He has very considerable intellectual powers, and exercises them only to do wrong, whenever he can do so with impunity. He was convicted of sheep-stealing.

Mr Combe's Remarks.—No. 4, E. A.—This individual possesses a very large development of the organs of the propensities, particularly of Combativeness, Destructiveness, Secretiveness, and Acquisitiveness, with decidedly deficient moral organs. The base of the brain is broad, and the coronal surface narrow. He is a bad subject; his dispositions are to cruelty and falsehood, and it will be extremely difficult to amend him.

Governor's Report.—No. 4, E. A.—This man has been confined for about four years, and for the greater part of that time has conducted himself *quietly*, and with apparent correctness; yet I am persuaded he is a very worthless character. He has tolerably good intellectual powers with respect to *matters of fact*, but slow at his books. He is mean and treacherous—will betray any of his fellow-prisoners to serve himself. He is an ill-looking fellow, but very amorous, and has frequently been detected in holding amorous correspondence with the female prisoners. I have not the least hopes of his reform—on the contrary, am persuaded that he is incorrigible. His crime was larceny.

Mr Combe's Remarks.—No. 5, A. M.—Enormous Destructiveness, Secretiveness, and Acquisitiveness; deficient moral organs: he would be a fearful thief, and cruel.

Governor's Report.—No. 5, A. M.—This boy came to me with a very bad character, which he has fully justified. His intellectual powers are of a high order, and he exerts them to the utmost to do all the mischief in his power.

He is without truth and probity, or good feeling of any kind, possesses great ingenuity, and is capable of framing a most consistent story with reference to acts in which he has been engaged, *not one of which* ever in reality happened. I think when he becomes a man, he will be a most dangerous character, and yet I fear he must be soon thrown back on society, as he is an orphan from Scotland, without a single friend to look after him. His crime was larceny.

Mr Combe's Remarks.—No. 6, M. M.—This woman has Cautiousness, Secretiveness, and Acquisitiveness, very large, with deficient Conscientiousness.

Governor's Report.—No. 6, M. M.—This woman is, in my opinion, worthless, and not likely ever to be reclaimed. She is mild and gentle in her manners with her superiors, and rather prepossessing in her appearance, but apt to quarrel with her fellow-prisoners. She is utterly destitute of truth, and abounds in low craftiness and cunning. She was convicted of larceny, and will probably pursue a course of theft.

Mr Combe's Remarks.—No. 7, M. G.—Destructiveness, Secretiveness, and Acquisitiveness, very large; Conscientiousness deficient.[1]

Governor's Report.—No. 7, M. G.—This girl is prepossessing in her appearance, gentle in her manner, and engaging in conversation; but I fear is, notwithstanding, very deficient in moral character. She was for a considerable time looked upon as superior to most of her fellow-prisoners, and treated accordingly; but was discovered,

[1] Quietness in prison, where temptation is removed and powerful restraints are imposed, may often appear where Combativeness and Destructiveness are large, if there be also good Secretiveness, Cautiousness, and Firmness to restrain them.

on being placed in trust-worthy situations, apt to betray her trust, and to show a disposition to pilfer, to aid others in pilfering, and to justify herself by false statements of facts. She was committed for larceny, and will, unless attended to by her friends, probably take to courses of impurity.

Mr Combe's Remarks.—No. 8, M. C.—This woman has large Acquisitiveness, Secretiveness, Wonder, and Imitation; but she has also the largest development of the moral organs of any whom I have examined. My impression is, that she would not commit crime in ordinary circumstances, and that there is diseased or irregular action of the brain.

Governor's Report.—No. 8, M. C.—This woman, for a long period after her confinement, was coarse, brutish, selfish, passionate, quarrelsome, and in all respects unamiable;—for the last year or two her conduct has been much better, and the violence of her passions much restrained, if not subdued. She has very considerable talent, and a strong mind, with powerful feelings, but has never turned either to any good account that I am aware of. I have always considered her as a very dangerous woman, and not at all likely to be reclaimed; but have always had a suspicion that the violence of her temper and frequent outrageous conduct were occasioned by bodily disease. For a long time after her confinement she complained of acute pains in her head, and showed evident symptoms of determination of blood to it. It is remarkable that the abatement of these pains, and the apparent subjugation of her passions, have nearly corresponded in time. I was much struck by Mr Combe's observations respecting this woman, as he guessed at once that which I had long known to be her bodily malady. She was convicted of larceny.

In addition to these remarks, the Governor in his letter states,—

"In the case of M. C., No. 8, it appears to me you have judged too favourably. This woman has been confined here for more than four years, and for a very considerable portion of that time her character was marked by violence, maliciousness, and brutality, and, when under the influence of high excitement, she was perfectly ferocious. I have frequently suspected that this ferocity of character was at least aggravated by a determination of blood to the head; and have been strengthened in that belief by finding that her violence of disposition became mitigated concomitantly with the abatement of very bad headaches with which she was for a time afflicted. I have been much struck by the fact of your suspecting organic disease in this woman, as coinciding with the result of my own experience, and I believe I may venture to state, with the opinion of our most respectable and intelligent medical attendant, Dr Charles Orpen."

In this woman, Mr C. found the organs of the animal propensities largely developed, which, when excited by diseased action, would render her ferocious as described; but the distinguishing characteristic of her head was the superior moral development, which, but for disease, would have sufficed to restrain and direct the propensities; so that the case forms no exception to Phrenology, but the reverse.

Mr Combe's Remarks.—No. 9, A. B.—This woman has very large Destructiveness, Secretiveness, and Acquisitiveness, with deficient Conscientiousness. She is a bad subject, and will with difficulty be amended.

Governor's Report.—No. 9, A. B.—This young woman has generally conducted herself well, though there have been some very flagrant exceptions. These exceptions appear to have been occasioned by *Temper*, as she is highly irritable, and is, when excited, frightfully furious and vindictive. She is perhaps the cleverest (intellectually) female

prisoner in the institution, and, although quiet, gentle, and correct in her ordinary demeanour, I fear she is not to be reclaimed. The crime of which she was convicted was grand larceny.

Mr Combe's Remarks.—No. 10, B. L.—This is a better subject than the last. She is milder and less coarse. She would probably swindle. By education she will be greatly improved.

Governor's Report.—No. 10, B. L.—This girl has been unusually well conducted, is prepossessing in her manner and appearance, docile, and anxious to receive instruction. I was very anxious to learn Mr Combe's opinion of her, and much gratified to find that it exactly coincided with that I had previously formed. She was convicted of stealing bank-notes.

Mr Combe's Remarks.—No. 11, P. T.—This is a tolerably good subject, and something may be made of him.

Governor's Report.—No. 11, P. T.—This young man's conduct since his admission has been generally correct. He does not appear to have strong passions of any kind, neither is his intellectual capacity great. His dispositions appear to be of a mild character, and I should be surprised to hear that he ever committed any flagrant crime; though, from a general *meanness* of character, I think he will, unless well attended to for some years, be guilty of petty thefts. I do not consider him to have strong moral feelings, and I know he was at one time addicted to lying. His crime was larceny."

It will have been seen that, in the opposition of physiologists to Gall's discovery, almost the only point in which the doctrine has been controverted by direct appeal

to fact, has been the assignment of the sexual instinct to the cerebellum; and so perseveringly have opponents contested this particular point, and so imperfectly and feebly, as it appears to the author, have they generally been met by phrenologists (when the firm ground upon which they rest, in this respect, is considered), that special attention has been bestowed upon this part of the controversy, in several of the preceding chapters; and, for the same reason, the writer will here apply the argument again, specially to the topic in immediate consideration.

The following history supplies an instance in many respects parallel to the experience of Mr Combe, as just recounted, but has regard only to the particular case of the cerebellum:—The account is taken from a work published a few years ago by Dr Felix Voisin, of Paris.[1] On the occasion of a visit to one of the prisons in Toulon, during the year 1828, Dr Voisin proposed to the governor, M. Reynard, that he should be allowed to predicate the probable cause of detention of the prisoners by judging of the predominant passion, as revealed by the cerebral development; stating that his object was purely experimental, being anxious, in this way, to test the accuracy of Gall's observations, and the probable justice of his conclusions.

"In hearing me speak thus," says Dr Voisin, "M. Reynard, an entire stranger to phrenology, seemed much surprised, and was himself solicitous for the trial. I made an engagement to return the next day, and at an hour mutually agreed upon, I found upon one of the quays within the prison walls, three hundred and fifty prisoners —forgers, robbers, and murderers. Among them had been mingled, at my request, twenty-two men condemned for rape. 'Seek these latter,' said M. Reynard to me, smiling, 'and if you find them, take down their numbers; I await you in my private office.'"

[1] De L'Homme Animal. Par F. Voison. Paris, 1839.

"I proceeded under the inspection of MM. Sper, principal surgeon to the navy at Toulon; Fleury, principal physician; Louvergne, surgeon-major; and Possel, curator of the museum. Without speaking, without uttering a single word, I examined the three hundred and seventy-two heads placed at my disposal; and every time that I found an individual presenting a broad and projecting nape of the neck, I made him go out of the ranks, and took his number. I thus put aside twenty-two persons, and my list being complete, I repaired in great haste to M. Reynard, impatient as I was to see in what manner an experiment made with the utmost impartiality, would decide upon one of the principal questions I was at that time investigating,—Has each predominant faculty in an individual, generally, an exterior sign on the head?"

"M. Reynard takes his list; I unfold mine; not without considerable excitement, I state the numbers which I have just inscribed; and it is to my own surprise, that of twenty-two individuals condemned for rape, and lost in a crowd of three hundred and fifty other criminals, I see thirteen revealed to me by a simple inspection of the cranium." . . . .

"'It is a very singular thing,' said the governor to me, 'the twenty-two individuals whom you have noticed, are not all condemned for rape, as I have just shown you; but I can attest that they are all of licentious manners, that for a long time they have been noted in my prison for being, in this respect, the objects of a most watchful surveillance, and that consequently the configuration of their heads has not deceived you in regard to the violence of their particular passion.'"[1]

Now, this experiment on the part of Dr Voisin, constitutes a very important fact, in relation to the disputed function of the cerebellum: it is difficult to see how any

[1] Op. Citat. pp. 97-99.

reasonable explanation of the result can be given, other than that which the phrenologist would offer. That, in a promiscuous assemblage of 372 prisoners, including 22 convicted of rape, 13 of the latter, or nearly two-thirds, should have been detected by their development of cerebellum; and that the remaining 9, mistakenly selected by Dr Voisin, should nevertheless have been notorious for their licentiousness, is a circumstance so decisive regarding Gall's physiology of the organ, that it is impossible to neutralise its force, except by proving the existence of some collusive deception in the transaction. This, however, will hardly be attempted. M. Lelut, whose anti-phrenological zeal induced him to write an octavo book against phrenology, most feebly strives to diminish its importance, but does not deny or seek to discredit it.

Those physiologists who maintain the muscular function of the cerebellum, constantly appeal to comparative anatomy, as exhibiting, in different species, an advance in the development and magnitude of the organ, correspondently with the increasing facility and readiness in the regulation of various combined movements. Now, if under different circumstances as to fundamental type, size influences power, surely the result should be still more decisive and marked, when the same elementary conditions are present, as in individuals of the same species; it is so certainly, with regard to other structures,—the muscles for example. Well then, if Flourens's physiology of the cerebellum be the true one, let his disciples show that individuals in whom it is large, excel coincidently in agility and dexterity. Let them supply the counterpart of the experiment just related. In the prosecution of such a step, let twenty-two individuals be selected, belonging to some class notorious for their cleverness in executing varied and combined movements,—the author would suggest the class to which the harlequin in pantomime belongs, whose facility in the maintenance of equilibrium is matter of constant experience.

Let then twenty-two harlequins (in plain dress of course) be mingled promiscuously with 350 other persons of nearly the same circumstances in other points of view; and let us see if, on an attempted identification of the former from the large development of cerebellum, so many as thirteen could be determined, as was accomplished in the parallel experiment by Dr Voisin. Or, if the broad and projecting basilar region of the head be not admitted as an outward indication of magnitude in the organ, let us imagine the accumulation of 372 *cerebella*, including twenty-two that had appertained to the class before supposed; now does any one think, that by employment either of the rule or the scales, the disciple of Flourens could detect thirteen out of the twenty-two? The author conceives, judging from his own experience, that, as a matter of fact, harlequins exhibit generally but a moderate development of the cerebellum.

In bringing this work to a close, the author will briefly recapitulate the argument by which he hopes, in some measure, to have accomplished the purpose with which it was undertaken. And he would here reiterate, that his intention has been, not to exhibit the brain and its physiology in any point of view involving extended or systematic detail, but to show rather the *method* by which the physiology of the brain should be determined. His first aim was to demonstrate the fallacy of certain modes of procedure too much in vogue, in this inquiry, amongst many able men,—to show that vivisections, comparative anatomy, and pathology, are all inadequate to the primary revelation of the functions of particular parts of the encephalon, however well adapted facts from these sources may be, in the corroboration and elucidation of inductions, gained by a process more just and philosophical. The exposition of this process became his next object; and, in this respect, he conceives himself to have shown, that coincidence of particular functional manifestation with particular structu-

ral development, in a single species, alone supplies decisive evidence concerning the physiology of different parts of the encephalon; and the defence of this proposition was succeeded by some account of the results to which the process had led. It was then shown, that such results harmonise completely with all sure information procured from collateral sources,—with every well-ascertained fact in the anatomy of the brain and nervous system and in mental philosophy, with all certain results of encephalic mutilation, with the knowledge furnished by researches in comparative anatomy, and with the irregular phenomena recognised in pathological investigations. Having advanced so far, he felt it right to dwell at some length on the intimate quality of structure as modifying functional manifestation, in order to show that what, in this respect, obtains in cerebral physiology, harmonises completely with all parallel circumstances in general physiology, and, indeed, in nature at large. In the present chapter, he has striven to establish that phrenology has just claims to rank amongst the inductive sciences, from the circumstance, that its facts admit of generalisation so as to evolve principles, or *inductions;* which latter he has attempted to distinguish from the *deductions* which, almost as a matter of course, phrenologists will varyingly make in tracing their science to its real or supposed consequences. And, last of all, he has indicated briefly the extent to which phrenology is susceptible of practical application. The author would here observe that, in the attempts which he has made to demonstrate the accuracy and the utility of the *results*, his leading purpose has always been to establish the soundness of the *method* by which they have been obtained, in realisation of the design professed on the title-page.

To determine the true method of ascertaining the relations subsisting between the structure and the functions of the encephalon, may, at first sight, have appeared so very simple a problem, as to have needed no *book* for its

solution; but the experience of the last fifty years affords a sufficient answer to this objection. In the year 1796, exactly half a century ago, Dr Gall publicly announced the discovery of a connexion between certain specific faculties of the mind, and certain particular parts of the cerebrum and the cerebellum; explaining the method by which he had attained his conclusions. As will have been apparent from what has preceded, a number of inquirers, from that day to the present, have pursued the method adopted by Dr Gall, and have obtained coincident results, have extended his discoveries, and applied them in practice. On the other hand, the great body of physiologists, rejecting the views of Gall, and condemning his method of investigation, have sought, by methods of their own, to arrive at a cerebral physiology different from that which he propounded. In reviewing the results of these fifty years' labour, it is impossible to deny that Gall's opponents, although many of them men of talent and boundless industry, have failed in the attempt to discover a physiology of particular portions of the brain, which has met with any very general approval, or which has been applied to any practical purpose. Satisfied that causes existed for so untoward a condition of cerebral physiology, it occurred to the present writer that some advantage would result from a systematic and extended inquiry respecting the merits of the various routes by which different physiologists have endeavoured to reach the goal.

The importance of this inquiry will be the more apparent, when it is considered that the method of investigation is recognised as of paramount importance in all other scientific inquiries; because it is constantly seen how the highest abilities, while pursuing an incompetent method, fail to accomplish what may be achieved without difficulty, by comparatively slender talents applied in the right direction. This truth has been perceived, and long and earnestly insisted upon by the followers of Gall, while it has been

almost always overlooked by his opponents. So early as 1819, Mr Combe, in a correspondence with Dr P. M. Roget, appended to his Essays on Phrenology,[1] formally proposed a discussion of this question of method, pointing out the superiority of the mode of investigation pursued by Gall, over that of all his predecessors or contemporaries; but Dr Roget did not meet the question with a due appreciation of its importance. In most of the phrenological works that have been published since that date, the question of method has occupied a prominent place, while it has been as regularly evaded or overlooked by writers on the other side. Indeed, so regardless have opponents been in regard to the competency of their own methods of inquiry, that the only point in which they have been consistent, is their rejection of that of Dr Gall. Every other method, however illegitimate, or however condemned by themselves in the abstract, has been received by the vast majority of them, as scientific, if it only professed to lead to conclusions subversive of his discovery. No rigid scrutiny has been considered necessary into either facts, principles, or deductions, if the aim have been to give the deathblow to phrenology. Facts the most inconsistent, inductions the most unwarranted by the premises, and arguments the most illogical, have passed unarraigned and uncondemned, when employed with a view to supersede Gall's physiology of the brain; and, as will have been obvious from many parts of this work, such a state of things has continued to a most unpardonable extent, even to the present day. Surely half a century is a period sufficiently protracted for inquirers to have wandered without profit in a trackless desert. If Dr Gall's method be fundamentally erroneous and impracticable, let its inadequacy be proved, and then let it be abandoned for ever; but, on the contrary, if it be sound, and constitute the best portal to the

[1] This correspondence is reprinted in Mr Combe's work, containing the translation of Gall on the Cerebellum, p. 217.

knowledge which is sought, let its utility at length be perceived and acknowledged, and let those who desire to enter the field of investigation, avail themselves of the opening which it affords, and proceed by the only practicable method to study and to advance the physiology of the brain.

Such are the views which prompted to the execution of the present work; of the imperfections which characterise many of the details and illustrations set forth in its progress, no one can be more fully sensible than the author; but of the justness and the soundness of all the leading arguments (in the employment of which he claims no originality), he is profoundly convinced. And, under all these circumstances, he ventures to assert that the time is come when, for many reasons, the opponents of Gall's physiology are bound to make good, or to abandon, their own methods of investigation, and to fix at the same time the true value of their own achievements in determining the special functions of the particular parts of the brain. Hitherto, they have considered it unnecessary to bestow much attention upon their own positions; they have principally aimed at negation. But as every erroneous doctrine obstructs the access of inquiring minds to truth, it becomes a useful labour to weigh the principles, to sift the arguments, and to try the facts of the opponents, so that the ore may be separated from the dross, and the latter removed from the student's path.

APPENDIX.

APPENDIX.

(Text, p. 154.)

ORGAN OF DESTRUCTIVENESS.

The following passages are taken from an able paper in the first number of the Phrenological Journal, published in 1823; and, as it is in the single instance of the organ of Destructiveness that the kind and extent of the proofs upon which is based the phrenological doctrine at large are exemplified, the subjoined extracts are given as an appendix.

After supplying an account of Gall's early observations with respect to this subject, coinciding with what has already been cited in the text, the writer of the article in question proceeds as follows :—

Dr Gall exhibits a plate of the brain of the lion and tiger, and of the calf and kangaroo, and the convolutions which are marked as the organ of Destructiveness exist in the former, while in the latter, they are absolutely wanting.

"At the same time," says Dr Gall, "it is proper to observe, that the organ is not, in all carnivorous animals, situated with rigorous exactness, above the external opening of the ear. Among some species of birds, for example, in the stork, the cormorant, the heron, the gull, &c., the external opening of the ear is considerably drawn back, and the organ of the propensity to kill is placed immediately behind the orbits, forming a large prominence upon each side, the size of which is found to bear a uniform proportion to the degree in which the animal manifests the propensity to kill. In comparing the

crania of carnivorous birds with the skulls of those that can live indifferently, either upon animals or vegetables, this prominence is found to be less conspicuous in the latter; in the duck, for example, and in the different species of thrushes; and it becomes less and less prominent in proportion as the birds exhibit a more distinct preference for vegetables, such as the swan, the goose," &c. The differences are illustrated by plates in Dr Gall's work.

These characteristics of development of the head vary not only in different *species*, but in different *individuals*, generally in proportion to the greater or less predominance of the carnivorous instinct. For example, the portion of brain, above alluded to, is perceptibly larger in the eagle and the falcon, than in the crow and the magpie, larger in the wolf than in the dog, and in the tiger than in the lion. In illustration of the difference among individuals, Dr Gall mentions, that he possesses a considerable collection of the heads of cats and dogs, in forming which, he paid particular attention to the degree in which the carnivorous instinct had manifested itself in each individual: and he states it as a fact, that in those cats which were ardent hunters, this region of the cranium is decidedly more developed than in those which were contented to receive their food in kitchens and parlours. The same circumstance holds in regard to dogs: those which had a large development of this part of the head were known to pursue and kill with avidity mice, rats, hares, and foxes, while those which were not naturally given to such pursuits, had a smaller development of the same part. He mentions, that it is impossible for any one to make a collection of the skulls of cats and dogs, paying attention to this difference of their dispositions, without being convinced, beyond the possibility of doubt, of the truth of these remarks.

So much for the lower animals: The following are a few of the facts stated by Dr Gall, in proof of the existence of the propensity and organ in man. The organ is situate,

in the human head, at the temporal and lower part of the parietal bone, and is about two inches and a half in length, and an inch and a quarter in breadth. This organ is very apparent in the crania of two of the accomplices of Schinderhannes, who had committed more than twenty murders. It was large in a soldier in Berlin, who experienced an irresistible propensity to commit murder, and who, upon the approach of paroxysm, of which he was sensible, before it attained its height, caused himself to be pinioned to prevent deeds of violence. It was found large in a young woman who had assisted her mother to murder her father :—In a young man, nearly an idiot, who had killed a child without any rational motive, under the impulse of a blind propensity :—It was large in the skull of a man named Homme-Dieu, exhibited by M. Brüggmans at Leyden. This wretch had precipitated a number of persons from the banks of the canals into the water, to enjoy their dying struggles.—It was also large in a man of Brunswick, who, without any other motive than the pleasure of killing, had committed two murders, the second on a child; also in twenty-five women, whom Drs Gall and Spurzheim found confined in different prisons, for infanticide :—It was large in a criminal of Frankfort, who was executed, after having committed a second murder; in Bouhours, who felled his victims with a mallet, to rob them of their money; in all the crania of murderers in the collections of Messieurs Haberl, Sax, Weigel. In Lepelley des Longs-Champs, the organ was largely developed, and the organ of Combativeness very little; and this person conceived the project of a murder, which he bribed Héluin, who was more courageous than himself, to execute. In the latter, the organ of Acquisitiveness was large. A man, named Valet, murdered his grandmother and three aunts; and Mercier, under promise of a sum of money, assisted him, by preventing the women from escaping, but without inflicting a single blow. In the cranium of Valet,

the organ of Destructiveness is well developed; in that of Mercier it is not so. In the latter, the organs of Combativeness, Cautiousness, and Benevolence, are very small, while the organ of Acquisitiveness is, on the contrary, very prominent. These skulls are preserved in the museum at the Jardin du Roi,* and Dr Gall possesses casts of them. The cranium of a man named Voirin, a hatter, guillotined in Paris about the year 1808, for having committed two murders, is exceedingly remarkable. The organ is much developed, and prominent. Dr Gall gives an extract from the act of accusation, which indicates exceeding barbarity in his conduct. The organs of Benevolence and Reflection were also small. The head of Dautun, who had murdered his brother, presents almost the same appearance. The skull of a criminal of Tarn, condemned to death on 21st January 1809, for having assassinated his brother-in-law, presents a very large development of the organ. Dr Coutèle narrates the circumstances, and certifies the development to be such as is now described. (*Observations sur la Constitution Médicale de l'Année* 1808, *à Albi. Seconde partie*, p. 163 and 165.) In the head of Madelaine Albert de Moulins, the organ is prodigiously developed, and this woman murdered, with a hatchet, her mother, brothers, and sisters. A cast of this skull is in the Phrenological Society's possession.

Dr Gall mentions also, that in the busts and portraits of Caligula, Nero, Sylla, Septimius Severus, and Charles the Ninth, this part of the head is represented as largely developed. Besides these, Dr Gall mentions several other cases, but the foregoing must suffice as an example of the kind of evidence on which he proceeded.

* We are informed by a gentleman, to whom casts of these skulls were shown by M. Royer of the Jardin du Roi, that he pointed out the characteristic indications of each precisely as here stated, without knowing that such a crime had been committed, much less the share which each had had in its commission.

In the *second* place, we shall state part of the evidence on which our own belief in this propensity and organ is founded. The following facts may be verified by any person who has a mind to inquire. The organ of Destructiveness is very large, and that of Benevolence small, in the skull of Bellingham, who murdered Mr Percival. The temporal bones protrude at least half an inch in the situation of the organ of Destructiveness, on each side, and the frontal bone presents a receding surface at the organ of Benevolence, where the skulls of individuals remarkable for benevolence generally rise into an elevation of half an inch or more. A cast of Bellingham's skull may be inspected in the Phrenological Society's collection. The organ of Destructiveness is also largely developed in the skull of Gordon, who accompanied a half fatuous pedlar boy, and, in the middle of a muir, beat out his brains with the heal of his clog, and robbed him of his pack, not worth twenty shillings. The skull itself is in the society's collection, and the bones protrude nearly half an inch on each side at the region in question. It is large in Charles Rotherham, who pulled a stake from a hedge, and beat out the brains of a poor woman on the highway, and robbed her of some very trifling articles. It is large also in the skulls of Hussey, Nisbet, and Lockey, who were executed for murder. It, and the organ of Acquisitiveness, appear to have been very largely developed in the head of Heaman, executed at Edinburgh for piracy and murder; also in the head of Robert Dean, executed for murdering a child, without any rational motive; and in the head of Mitchell, executed for murdering a young woman whom he had seduced. In the heads of David Haggart and Mary Mackinnon, executed at Edinburgh, and of Booth, a poacher, executed at York, all for murders committed on the impulse of the moment, it appears considerably developed; while in them Combativeness is also very large.

In the whole of these heads and skulls now enumerated,

the distance, in a direct line, measured, by means of callipers, from the external opening of the ear to the middle of the surface of Philoprogenitiveness on the skull, *i. e.* about half an inch above the spinous process of the occipital bone, is equal to the distance from the external opening of the ear to the external surface of the head at lower Individuality, corresponding to the top of the nose ; and *the coronal surface is narrow.* This indicates a great preponderance of the animal organs situate in the lower and back part of the brain, over the organs of the moral sentiments, and of intellect, situate in the coronal and frontal regions of the head. On the other hand, in several hundred individuals of gentle dispositions and good intellects, whose heads we have examined, we found, with few exceptions, the distance before the ear, according to the above measurement, to exceed the distance behind it to a considerable extent, in many cases amounting to an inch, and *in every instance*, the coronal surface was large and ample, in proportion to the base and posterior part of the brain.

Dr George Murray Paterson, surgeon in the Honourable East India Company's Service, mentions, as the result of three thousand actual examinations, that the organ is small in the heads of the Hindoos in general ; who are known to be extremely tender in regard to animal life. In the skulls of fourteen Hindoos, twelve of which were presented to the society by this gentleman, and two by Dr Combe of Leith, the development of the organ will be found to be decidedly less than in the skulls of Europeans in general.

Several years ago, Peter Somners was tried before the High Court of Justiciary, and found guilty of wantonly murdering, in a fit of intoxication, an old man with whom he was amusing himself on the road. We were informed by a gentleman, who had an opportunity to know the fact, that this young man had manifested great cruelty to animals at previous periods of his life. We saw him in prison, and his organs of Destructiveness were very large.

In the country, we saw a boy who had watched the progress of a brood of swallows, and when they were fully fledged, delivered them alive, one by one, into the mouth of a sow, without any other motive than the barbarous pleasure of seeing them devoured; and in him the organ was very large. We have read a full account of this case in the society's MS. book of reports, which we found open to public inspection in the society's hall. In the collection of Dr Barclay, there is the skull of a Negro who committed several murders, and in it Destructiveness is very large.

Hitherto, however, we have contemplated Destructiveness only when acting with excessive and uncontrolled energy, and producing abuses of its legitimate function. We have seen it raging, in brutes and in man, "without check or limitation, without either pity or remorse." It is quite obvious, that it was precisely in such cases that the organ and the propensity were most likely to force themselves upon the notice of the observer, because they were present in that high degree of development and activity, which produced a predominance of this feeling over the other faculties of the mind. Destructiveness, however, when directed by the higher sentiments, serves a valuable purpose in the mental economy. The form in which it manifests itself when opposed by obstacles from without, is the passion of anger. When combined with benevolence, or a strong sense of justice, it gives rise to a virtuous indignation, some degree of which is absolutely necessary to the true dignity of man. Nothing is more necessary or more becoming a perfectly virtuous character, than a just degree of severity and anger against every species of vice, fraud, deceit, and cruelty. When we witness any signal instance of these, not to be angry is a proof of a mean and contemptible spirit. It is this faculty which gives to the character its greatest energy and power. It lends a peculiar force to the accents of command. Every

command so enforced implies in it a threat: "Do thus, or thus, as ye shall answer." It is an intimation of the will of the speaker, coupled with the further intimation, expressed or implied, that disobedience will be attended with fatal or inconvenient consequences. This power, accordingly, is highly necessary to the chiefs of savage or uncivilised nations, and even among a more refined people to all in situations of command. Robert Bruce in former days, and Buonaparte in our own, had this organ largely developed. The cast of the skull of Bruce is in the collection of the Phrenological Society, and may be inspected by those who wish to verify the statement. Destructiveness also gives edge to sarcasm and satire, and prompts the fancy to the conception of all those images of terror, which become sublime or horrible, according as they are clothed with ideality, or presented in naked deformity. Now, we state as positive fact, that we have measured with callipers, and noted in inches and tenths, the development of this organ in a great variety of individuals, and that we have found the presence of the peculiar kind of energy now mentioned, to bear a regular proportion to its size. In several eminent public characters in particular, whose head we have examined, but whose names, for obvious reasons, we forbear to mention, who manifest, in a striking manner, this mental quality, we found the organ large, and we never found a single individual who manifested this power, in whom the organ was small. When, on the other hand, the organ of Destructiveness is small, and the higher sentiments are powerful, there is a want of fire in the character: there is a softness which is little fitted to awe or control a fiery spirit in others,—an effeminacy which does not make itself felt in the contests of life,—and a tendency to listless insipidity, from the want of a spur within; and those characteristics are greatly aggravated if Combativeness also be small. In private life, we have met with individuals who were noted for this undue soft-

ness and effeminacy of disposition, who, with fair talents, were unable to make themselves felt in the circles in which they moved, and whose own exertions were never able to carry them through the difficulties of life; and in them we found the organ in question small. These differences amounted to at least half an inch on each organ of Destructiveness, or a whole inch on the general breadth of the head across the temporal bones.

The facts now adverted to may be viewed as positive proofs of the existence of this organ and faculty; but, in the *third* place, we proceed to advert to some phenomena in human nature, recorded without any view to phrenology, from which some inferences may be drawn concerning the existence of this propensity. We may premise, that metaphysicians and ordinary observers of human nature, admit the existence of instinctive tendencies in the human mind, quite distinguishable from mere intellect or reason. Thus, for example, no one confounds the feeling of love with mere intellectual ideas. The intellect perceives the objects which excite this passion, but the feeling itself is not intellectual. The same observation applies to the sentiments of hope, benevolence, fear, and many others, which are generally admitted to be different from, and not immediately dependent upon, the intellectual powers. Now, these feelings are known to become diseased; and the effect of the diseased excitement on the feelings of love, for example, is to produce ungovernable desires,—on the sentiment of hope, to give rise to extravagant joy, without any adequate external cause. But in these cases, a sane feeling is admitted to exist, which disease excites into inordinate activity, but does not create. Now, if we find patients under mental alienation displaying the most irresistible impulse to destroy, we are entitled to argue, upon every principle of analogy, that some propensity of a similar character must exist also in the same state, of which these

manifestations are only abuses caused by the disturbing influence of disease;—in short, that disease can no more produce, as a first cause, a violent tendency to destroy, than it can create a sixth sense, a new sentiment, or an intellectual power. Is there, then, such a tendency manifested in derangement or not?

The following cases are published in Dr Gall's Physiologie du Cerveau. Mr Mayer of Berlin, surgeon of a regiment, showed Drs Gall and Spurzheim, in presence of Messrs Heim, Formey, Gœricke, and others, a soldier, who, from distress at the loss of his wife, had suffered a wasting of the body, accompanied with excessive irritability. At last he was seized every month with convulsions. He perceived their approach, and as he felt an immoderate propensity to kill increasing upon him, in proportion as the paroxysm approached, he begged his attendants to bind him with chains. At the end of a few days the fit and propensity diminished, and he himself indicated the time he might with propriety be set at liberty. At Haina they saw a man, who, at certain periods, felt an irresistible desire to commit violence on others. He also was aware of his unhappy tendency, and made himself be confined till such time as he felt that he would not abuse his liberty. A man, subject to melancholy, assisted at the execution of a criminal, and the spectacle excited in him such a vivid emotion, that he was instantly seized with a vehement desire to kill, and at the same time preserved the greatest abhorrence of such a crime. He spoke of his unhappy condition with tears and extreme confusion, begged of his friends to save themselves from his hands, and thanked them when they strenuously resisted his attempts. Pinel, also a celebrated writer on insanity, who is not a phrenologist, remarked that patients are frequently furious without derangement of the intellectual faculties, and he therefore testifies dissatisfaction with the definition which Locke has given of insanity. He speaks of an in-

dividual who was subject to periodical attacks of mania, and whose fits regularly returned after intervals of calm of several months' duration. "Their approach," says he, "was preceded by the sensation of a burning heat in the interior of the abdomen, then in the breast, and latterly in the face; next redness of the cheeks, sparkling eyes, great distention of the veins and arteries of the head, and at last an uncontrollable fury, which inspired him with an irresistible propensity to seize an instrument or offensive weapon to knock on the head the first person who presented himself to his view. He experienced a sort of internal combat between this ferocious impulse to destroy, and the profound horror which rose in his mind at the very idea of such a crime. There was no mark of wandering of memory, imagination, or judgment. He avowed to me, during his strict seclusion, that his propensity to commit a murder was absolutely forced and involuntary,—that his wife, whom he tenderly loved, had nearly become his victim, he having scarcely had time to bid her fly to avoid his fury. All his lucid intervals were marked by melancholy reflections and expressions of remorse; and so great did his disgust of life become, that he had several times attempted, by an act of suicide" (this is common in the excess of Destructiveness), "to bring it to a close. What reason have I (said he) to cut the throat of the superintendent of the hospital, who treats us with so much kindness? and yet in my moments of fury I am tempted to rush upon him, as well as others, and plunge a dagger in his bosom. It is this unhappy and irresistible propensity which reduces me to dispair, and makes me attempt my own life." (*Sur l'Alienation Mentale, deuxième édition*, pp. 102 et 103, § 117.) "Another insane patient," says Pinel, "experienced attacks of fury, which recurred periodically during six months of the year. He was sensible himself of the decline of the symptoms towards the end of the fits, and indicated precisely the time when he

might be set at liberty in the interior of the hospital. He requested the attendants to defer his liberation, if he felt doubts of controlling the blind impulse which prompted him to commit acts of the greatest violence. During his intervals of calm, he avowed that while the fits lasted, it was impossible for him to repress his fury; that if any one presented himself before him, he experienced, believing that he saw the blood flowing in the veins of the man, an irresistible desire to suck it, and to tear his limbs to pieces with his teeth." *Ibidem*, p. 283, 284, § 239.

The following case is given on the authority of Dr Zimmerman of Krumbach. "A peasant, born at Krumbach, in Suabia, aged twenty-seven, and unmarried, and of parents who did not enjoy the best of health, was subject, from the age of eight years, to frequent attacks of epilepsy. About two years ago his malady changed its character, without its being possible to assign any reason for the alteration;—instead of the attack of epilepsy, the man, since this period, felt himself seized with a violent propensity to commit a murder. He felt the approach of the attack sometimes several hours, sometimes a whole day, before its actual invasion. From the moment in which he felt the presentiment, he earnestly entreated to be chained, to prevent him from committing so fearful a crime. 'When the propensity seizes me (said he), I am absolutely forced to kill or strangle, were it even an infant.' His father and mother, for whom he felt a sincere attachment, would have been, during the attack, the first victims of his propensity. 'Mother,' cried he, in a tone of voice truly terrible, 'fly, or I shall be absolutely forced to strangle you.'"

M. Fodéré, also, who is rather an opponent than a convert to phrenology, cites several examples of the great activity of the destructive propensity in mania; among others, that of a young man, of twenty-five years of age, who had several times lifted his hand against his father,

and who was shut up on this account in a lunatic asylum. He was always very neat in his person, and appeared sensible, "which induced me," says Fodéré, "to endeavour to excite in him a feeling of remorse; but he would never admit the enormity of his crime, and he frequently eyed me to give me a blow,—his manners all the time being extremely polite." *(Traité du Délire appliqué à la Médecine, par M. Fodéré, tome premier*, p. 401, § 196.) Other cases are recited by Pinel, pp. 119, 120, and 165, which it is unnecessary to detail.

These cases might be multiplied to a great extent, by mere reference to the public newspapers. We shall cite only one, which appeared in them in May 1822. "MURDER OF A CHILD BY ITS MOTHER.—On Sunday morning, about half-past ten o'clock, a most sanguinary and horrid murder, of unparalleled inhumanity, was perpetrated on the body of a fine female infant, about eight months old, named Sarah Mountford, by her own mother, wife of Mr Mountford, weaver, No. 1, Virginia Row, Bethnal Green. The husband, who is a methodist, had gone to chapel, leaving his wife to clean, and send to the Sunday school, her young family. Having done this, it appeared she cleaned herself and her infant, when, overcome by some extraordinary aberration of intellect, she cut off the head of the child with a razor, and, besmeared with the blood, immediately told the persons in the house of the bloody deed, desiring to be given into custody, as she wanted to be hanged. From the conduct of the wretched woman after the transaction, no doubt can be entertained of her insanity. Mrs Mountford underwent a short examination on Monday, and was committed for trial. A coroner's inquest has since been held, which returned a verdict of wilful murder against the wretched woman. The distress of the family is extreme. The unhappy husband and two of the eldest daughters are seen running about the streets in a state of distraction. One of the latter has been de-

prived of utterance since the horrid transaction." This woman is said to have been "overcome by some extraordinary aberration of intellect;" which mode of expression may be forgiven in the writer of a newspaper paragraph, but, viewed philosophically, it is absurd. The intellectual powers enumerated by the metaphysicians, such as perception, conception, memory, imagination, and judgment, furnish no propensities to action, which, being deranged, could produce such a piece of barbarity. Derangement of intellect causes the patient to reason incorrectly, and speak incoherently; but, if his *feelings* be sound, he is not mischievous. Here, however, the unhappy woman seems to have been inspired with a blind and irresistible impulse to kill.

FINIS.

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